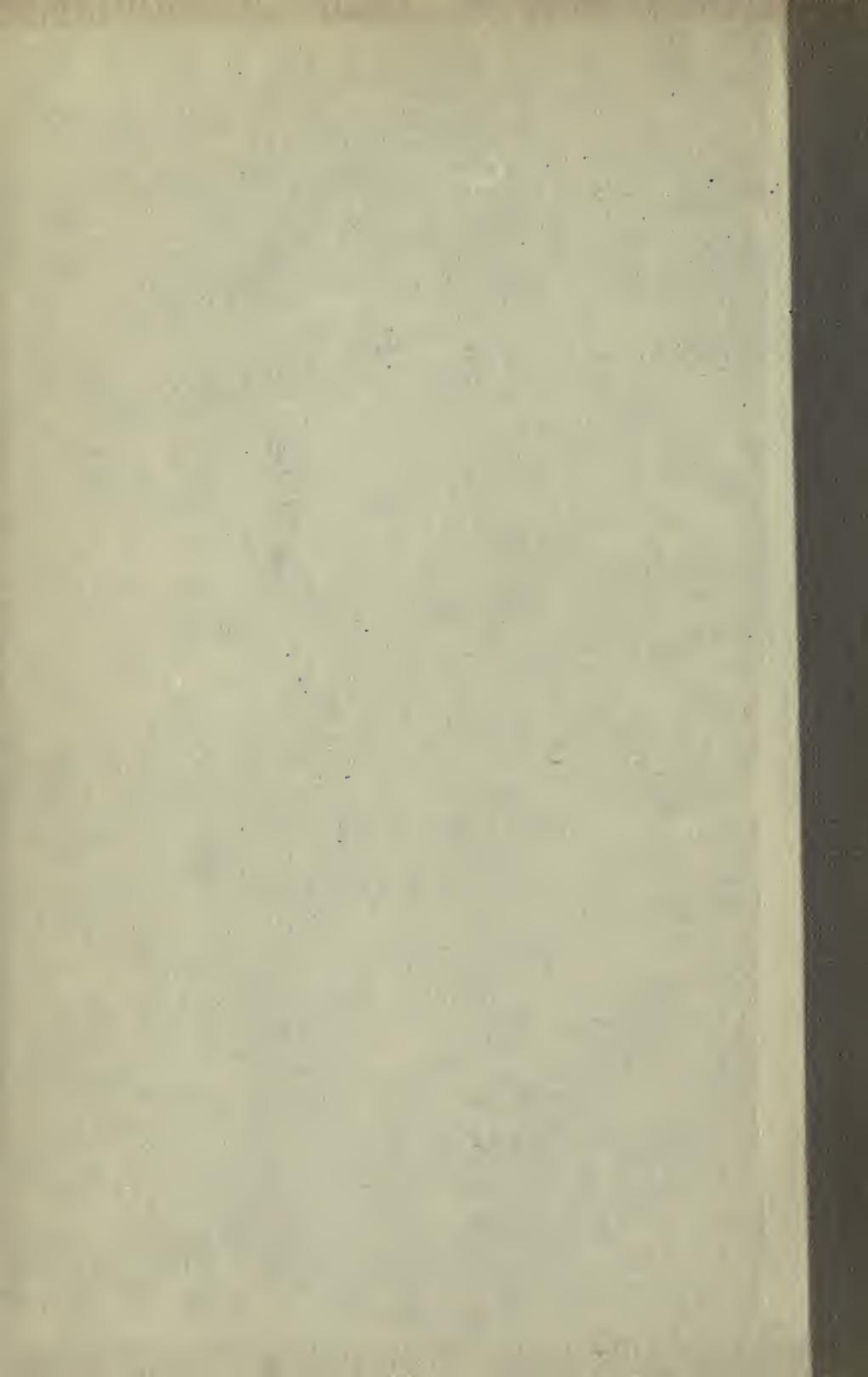
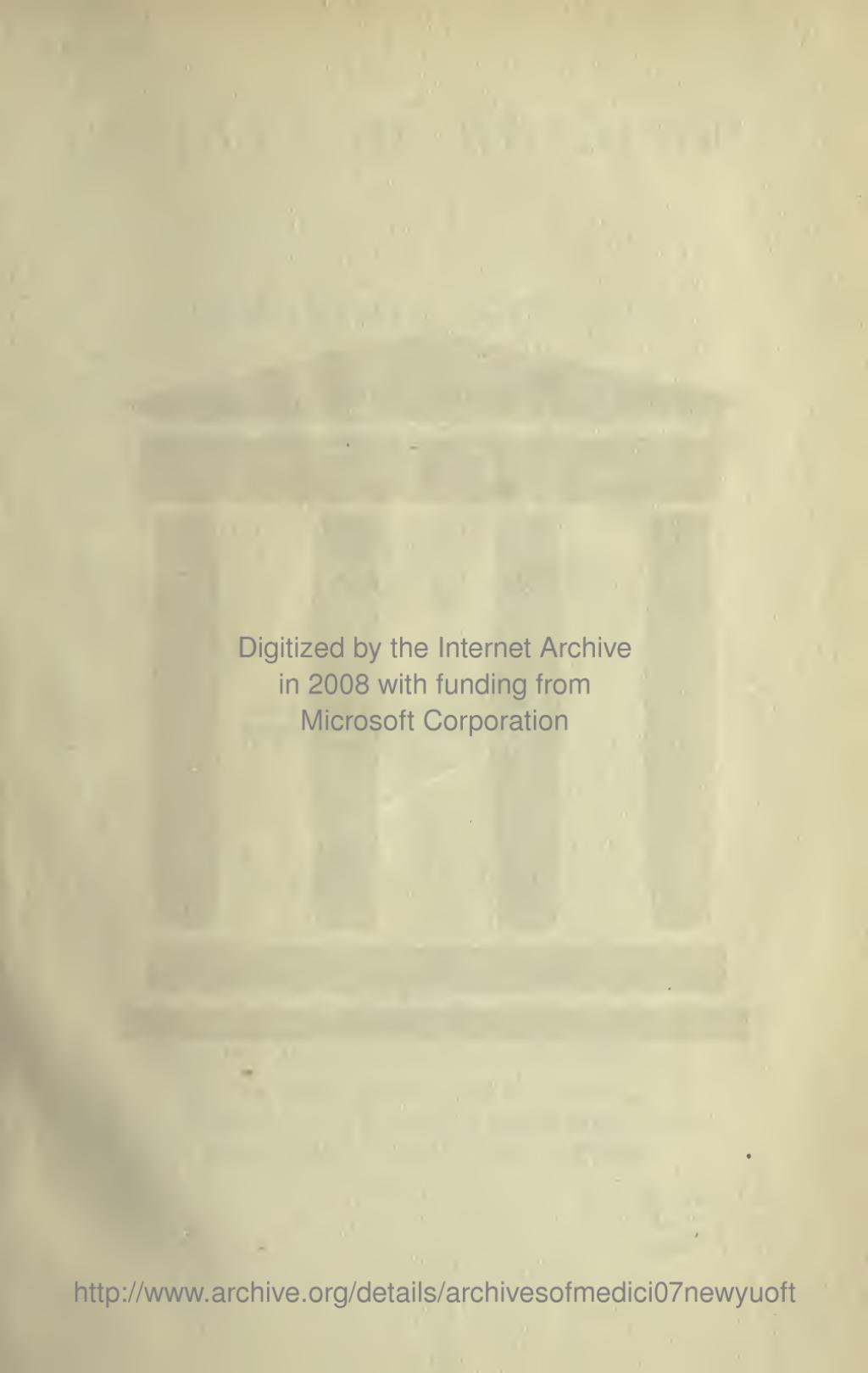


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# Archives of Medicine

A BI-MONTHLY JOURNAL

DEVOTED TO ORIGINAL COMMUNICATIONS ON MEDICINE,  
SURGERY, AND THEIR SPECIAL BRANCHES

EDITED BY

E. C. SEGUIN, M.D.

S'il est possible de perfectionner l'espèce humaine, c'est dans la médecine qu'il faut en chercher les moyens.

—DESCARTES

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# ARCHIVES OF MEDICINE.

## Original Articles.

CARCINOMATOUS PAPILLOMA OF BLADDER.  
OCCLUSION OF ORIFICES OF URETERS.  
DEATH BY URÆMIC COMA.

BY SAMUEL B. WARD, M.D.,

ALBANY, N. Y.

Col. J., American, married, 55 years of age, of healthy parentage and relations on both sides, has never had any serious illness, nor met with any injury; has always been of perfectly correct habits in every respect; smokes moderately and drinks an occasional glass of wine.

Eighteen years ago he had an attack of severe pain in the left side of the abdomen, the paroxysm lasting about six hours, and attributed by his attending physician to the passage of a renal calculus. With this exception, he does not remember to have had a pain or an ache in his life until his present illness.

About ten years ago he began to have hemorrhoids, which, about six years ago, commenced to bleed and annoy him; but he could, and always did, return them when they protruded. This summer (1880) they had to be returned a dozen times a day, and he then placed himself under the care of a physician in Bridgeport, Conn., and in the course of five or six weeks was cured by what he calls a "process of absorption." He has since been entirely free from all annoyance from this source.

In Sept., 1878, when in Geneva, Switzerland, he passed, at one single urination, dark-colored urine which he considered to be bloody. The urine was clear the next time he emptied his bladder, and so remained until the second week in November, when, at Nice, he passed a larger amount of blood than at Geneva. The

next urination was again clear, and nothing further was noticed until, in going through the Pitti Palace, in Florence, in December, he experienced a sharp, burning pain in the end of the penis, which persisted for a day or two. He consulted Dr. Young, who said that the difficulty was due to the passage of uric acid crystals, and gave him alkaline treatment with *the result above stated*.

In Feb., 1879, at Rome, the urine remained dark for about a week, and deposited a mahogany-colored sediment. He was relieved by capsules of copaiba. In Leipzig, the first week in April of the same year, after a long carriage-ride over the pavements, he passed a larger amount of blood than ever before, but at no time suffered any pain while passing the blood. In Paris, the second week in April, the urine again became bloody, and so continued for about three weeks.

On April 28, 1879, he was carefully examined by Dr. Reliquet, who made the following diagnosis :—“Hémorrhoides. Congestion prostatique consécutive. De là le sang évacué spontanément avec l'urine.” Dr. R., after careful examination, definitely excluded stone. On the theory of the diagnosis he was ordered to take Hunyadi Janos water every morning before breakfast, and a large rectal injection soon afterward, and sulphate and bicarbonate of soda three times daily. Thus the bowels were kept quite free and the urine alkaline; but on June 3d the urine again became bloody, and so continued at pretty short intervals until Sept. 15th. Then it gradually diminished and the urine was only bloody for a day or two at a time at intervals of about a month, and has so continued ever since. A long railroad ride would always bring it on.

Up to July 10, 1880, he had never had any pain or soreness connected with urination, except as above stated at Florence. At this time he began to have, at intervals, scalding pain the whole length of the urethra, accompanied by a desire to pass urine, and more or less straining. Urination was followed by an uncomfortable aching, which lasted five to ten minutes. Belladonna suppositories afforded great relief. This condition of pain and aching persisted and increased in severity up to the time that I first saw him, Oct. 7, 1880, when he was suffering severely and almost constantly.

In December, 1879, he first noticed an increased frequency in urination, and this became more and more of an annoyance until the latter part of August, 1880, he was passing water about every fifteen minutes during the day, and was obliged to get up at about the same intervals during the night, though toward morning he

might sleep an hour and a half or two hours. About the middle of September he began to pass urine involuntarily and during sleep unconsciously.

Oct. 7, 1880.—Saw him late in the evening for the first time professionally, and found him looking haggard and worn-out from loss of sleep and constant suffering. Urine dribbling constantly and tenesmus well marked. As he thought that morphine affected him unpleasantly we gave him codeia for the night.

Oct. 8th.—Had a much more comfortable night than usual, as far as the amount of sleep was concerned, but dribbled constantly and is still suffering as before. Put him upon half-drachm doses of lactic acid and washed out his bladder with a five-grain solution of the same.

The urine examined this morning contained quantities of pus; no considerable amount of blood; no casts; many crystals of triple phosphates; sp. gr. 1,008; reaction neutral; no sugar; a great deal of mucus; considerable epithelium; and after boiling, the addition of nitric acid, and standing six hours, the precipitated albumen occupied one third of the bulk of the urine used.

Oct. 11th.—The pus in the urine has largely decreased in quantity, and there is notably less mucus and epithelium, while its reaction is markedly alkaline.

Up to yesterday the patient kept passing gravel in small masses every hour or two, which on examination proved to be triple phosphates. The bladder has been washed out every day, though the process is more painful than with most patients. He says that his suffering is entirely changed in character and very much diminished, and the improved appearance of his face certainly corroborates his statement.

Oct. 12th.—Sounded him for stone with an entirely negative result. The passage of the steel instrument was effected without the least difficulty, and caused him very little pain; but on its withdrawal about an ounce of clear blood ran from the urethra and formed a firm clot on a cloth which received it.

Oct. 15th.—The constantly wet cloths in which the dribbling urine has been caught have made the head of the penis and the prepuce very sore, and the patient has therefore occupied the recumbent posture for the past twenty-four hours, and the urine has all been caught in a paper basin. We have thus been able for the first time to measure it accurately, and find that it amounts to 157 ounces, a larger quantity than we had supposed. There is very little pus now, but considerable ropy mucus the passing of which

causes almost as much pain as did that of the gravel. No great change in the urine except that to the naked eye the pus has almost disappeared, while the microscope still shows a few cells and some blood.

Oct. 16th.—In washing out the bladder to-day my attention was attracted by some small masses of tissue (apparently) floating in the fluid which now runs almost clear. These masses were examined by myself, and also submitted to Dr. Wm. Hailes, Prof. of Histology and Pathology in the Albany Medical College, who reports that "they are composed of papillæ, consisting simply of connective tissue, forming a support for capillary vessels which have their embryonic walls and terminate in loops in the ends of the papillæ." Neither he nor I could find any thing which was characteristic or even suspicious of malignity. Fig. i is a very accurate reproduction of the appearance of one of the portions examined, and all the rest were entirely similar.

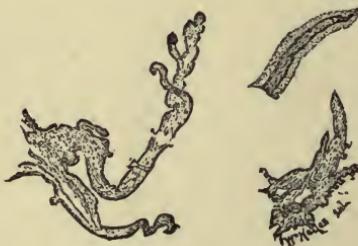


FIG. I.

Several detached papillomatous growths found floating in specimens of urine. They consist principally of connective tissue, forming a support for capillary vessels traversing the papillæ, and resemble benign papillomatous growths.

Rectal examination shows the mass to lie above the prostate, to be of considerable size and occupy a portion at least of the sides as well as the posterior wall of the bladder.

Oct. 18th.—The quantity of fluid passed from the bladder has diminished to 113 ounces in the past day, having been yesterday 125, and the day before 130. On the 16th the patient suffered so severely that he was obliged during the day and night to take three quarters of a grain of morphine. His stomach was disordered thereby, and his tongue, which has been thickly covered with a brownish fur all the time, was worse than ever. Pulse, which has ranged from 100 to 120, is to-day 105. The temperature has been at all times from 98° to 99°.

Careful physical examination of the thoracic and abdominal

cavities reveals nothing. To-day he has taken no morphine ; has been more comfortable than usual ; has suffered no pain except when passing the little masses of tissue above referred to, or the ropy mucus, and the latter has still further diminished.

Repeated examinations of the fragments of tissue passed show no change in their character, and the diagnosis, therefore, seems justified of simple papilloma of the bladder.

Oct. 21st.—Mr. J. has gradually and steadily improved ; eats and sleeps better ; has much less pain ; and the amount passed from his bladder has diminished to 87 ounces. The amount of albumen varies from one fourth to one third, and no casts can be found at any time. The lactic acid has been increased to drachm doses three times daily, and the washing out of the bladder has been discontinued, because the operation causes him so much distress at the time and for an hour or two afterward, while the disappearance of pus from the urine has done away with the necessity.

To-day Prof. Wm. H. Van Buren saw the case with me in consultation, verified the diagnosis of vesical tumor, advised against an operation, but recommended the use of *thuya occidentalis*, from which he had derived much benefit in similar cases. He also agreed with me that notwithstanding the presence of large quantities of albumen in the urine there was no good reason to suspect any disease of the kidneys.

Nov. 5th.—Since last date the patient has eaten and slept well, is cheerful and bright, and but for his local trouble would feel as well as ever.

The amount of fluid passed from his bladder has varied from 106 to 76 ounces ; the proportion of albumen from one to two fifths ; no casts at any time ; occasionally a little blood ; sometimes a few pus corpuscles, sometimes almost none ; there is always considerable ropy mucus entangling quantities of crystals of triple phosphates. The urea has been frequently estimated by the hypobromite method, and has varied from .50 to .59 of one per cent. No more villi have been found though they have been carefully searched for. The incontinence of urine is a great annoyance to him.

A faithful trial was given to the *thuya* but without any improvement in the condition of the urine, or any relief to the patient. In fact he suffered so much more than when taking the lactic acid that he asked to go back to the latter, and did so with decided relief.

He has now almost no vesical tenesmus, but suffers pretty sharply when passing the masses of semi-coagulated mucus with the crystals entangled in them, which seem to scrape and irritate the mucous membrane of the urethra.

Dec. 19th.—There has been little change in the patient's general condition since last date, but he is somewhat better. For a few days he has sat up in a chair, partially dressed, for about half an hour at a time, and eats very well. The constant dribbling of urine is a great annoyance to him whatever position he may assume. He has lost some flesh, but has gained in strength.

No change worth noting in the condition of the urine. Though the specific gravity remains low and the proportion of albumen very considerable, there are no casts at any time. The daily amount has averaged 85 ounces, with a minimum of 50 and a maximum of 108. Another trial has been made of the thuya with the same result as before, and the remedy which gives him the most comfort is, without doubt, the lactic acid. He suffers most when passing only a small amount of fluid; and at such times digitalis and acetate of potassa or other diuretics always afford relief. The infusion of digitalis was the most reliable, but could not be long employed at a time on account of its interference with the action of the stomach.

Jan. 28, 1881.—No change in the urine worth noting. Patient has had three hemorrhages from the bladder, one of which was quite copious. The blood coagulated in considerable amounts in the bladder, and the passing of the clots was the occasion of paroxysms of pain so severe as to demand the hypodermic injection of morphine, in half-grain doses at night, to procure any rest.

The family are exceedingly desirous that an operation should be done to remove the tumor, if not accompanied by too great risk. They are driven to this by the agony which the patient suffers, and are encouraged by reports which they have heard of success in cases which are narrated as being of a similar character. I therefore yesterday consulted Dr. Van Buren again as to the propriety of trying it. The arguments in its favor are that the patient has an excellent constitution and is in very fair general condition; the only fragments of the growth which we have been able to obtain have proved to be simple papillomatous villi, without any evidence of malignity; and the statistics of the removal of such growths are somewhat encouraging. Dr. Van Buren was opposed to the operation on the grounds that the growth was too large to be entirely removed, and that he was sat-

isified from the history of the case that the base of the growth was malignant. He thought it possible that at some future time the vesical orifice of the urethra might become occluded, and he would then advise the opening of the bladder, by the usual perineal operation, for the purpose of giving exit to the urine through a permanent fistula. If this had to be done he would then advise the removal, by tearing and scraping, of as much of the tumor as possible. Dr. Van Buren's advice was followed and no operation was attempted.

Feb. 24th.—The patient's sufferings have been relieved by morphine as demanded. He is worse in every way; the morphine interferes with his appetite and digestion and causes nausea and vomiting, though not as much as opiates given in any other way. Codeia and other drugs which do not interfere with the digestive apparatus exert no controlling influence over the pain. The morphine has been so objectionable that I gave to-night in its place fifteen grains of chloral hydrate with twenty of bromide of potassium.

Feb. 26th.—Mr. J. rested very well after taking the chloral night before last. Yesterday he suffered less pain than usual, and was notably dull and sleepy, though he could at all times be aroused and would then talk rationally and cheerfully. The drowsy condition was attributed to the chloral.

Last night, without chloral or any anodyne, he rested well, but this morning the drowsiness, which has considerably deepened, can not be attributed to any drug. An examination of the account kept of the amount of fluid passed from his bladder showed that it had diminished to an average of less than 55 oz. per day, and though there is no oedema of any portion of the body, there seems little doubt that the condition approaching coma in which he has lain for the past two days is due to uræmia. The administration of diuretics and sudorifics was therefore begun and a brisk cathartic given.

March 1st.—In consequence of the treatment the flow of urine has increased to an average of over 60 oz., and the bowels have moved freely. Of course the patient's condition is correspondingly better.

There seems no satisfactory way of accounting for the uræmia except on the supposition that the growth has occluded the ori-fices of the ureters and thus made pressure backward through them on the kidneys, impeding their action.

March 8th.—Up to the 5th inst. Mr. J. remained in very fair con-

dition, but suffering intense pain from the passage, at short intervals, of masses of coagulated mucus entangling large numbers of crystals. For the past few days the amounts passed have been as follows : March 2d, 47 oz. ; March 3d, 30 ; March 4th, 32 ; March 5th, 33 ; March 6th, 24 ; March 7th, 14 ; and to-day scarcely any—all this in spite of every effort made to effect an increase. Since the 5th he has been semi-comatoso nearly all the time, aroused only when spoken to or by the paroxysms of pain. The suffering was so great that death, which came at 11 P.M., was almost a relief.

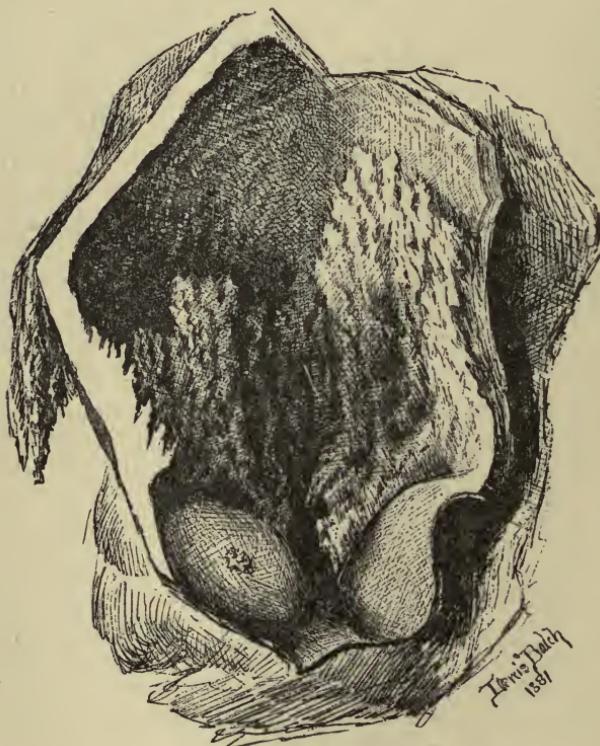


FIG. 2.

Gross appearance of bladder laid open by incision in anterior median line. Drawn by Dr. Lewis Balch.

*Autopsy.*—4 P.M., March 9th, 17 hours after death. Present, Drs. Vanderpoel, Sr. and Jr., Dr. A. Van Derveer, and Dr. J. S. Mosher.

General appearance, well-nourished. *Rigor mortis*, well-marked.

By request of the family the head and thorax were not opened. Abdominal cavity opened by crucial incision. Omentum healthy and contained a fair amount of fat. Intestines healthy.

Bladder removed and opened by anterior vertical incision. Walls thickened; interlacing columns, formed by hypertrophied muscular bands, well marked; mucous membrane at the base covered with a soft cauliflower growth, extending all around the neck and up on each side—rather more on the left than on the right—so that one half, or perhaps a little more, of the internal surface of the bladder was covered with it. Orifices of the ureters entirely concealed by the growth and their lumen obscured by the neoplasm.

Both ureters were filled their whole length with urine and dilated to about half an inch in diameter, so as to easily admit the end of the little finger.

Left kidney: pelvis very much dilated, connective-tissue capsule quite adherent, cortical substance and pyramids apparently free from disease. Right kidney: apparently healthy, capsule not adherent, pelvis greatly dilated.

Not the least trace of disease was found in any of the other abdominal organs.

The heart was examined through an opening in the diaphragm and found normal.

Fig. ii, for the drawing of which I am indebted to my friend, Dr. Lewis Balch, gives a very correct idea of the appearance of the bladder and extent of the disease.

*Remarks.*—The most important point to determine in this case, in relation to advising an operation, was the exact character of the growth. There was no history of malignant disease in any individual of any branch of the family. The patient himself was entirely free from any cachexia; had, as far as careful examination could detect, no disease of any other organ; had always been perfectly healthy, with the exception of the supposed passage of a renal calculus eighteen years ago, which had never been repeated, and the hemorrhoids of which he had been entirely relieved when I first saw him. His bladder trouble dated back just two years to a passage of bloody urine, and that had been

repeated on numerous occasions. There had been entire absence of the pains said to be characteristic of malignant disease, indeed there had been no pain worth mentioning until the attack of acute cystitis during which I first saw him. Then portions of the mass obtained in washing out the bladder and subsequently in the urine proved, on careful microscopical examination, to consist of simple papillomatous villi, without a trace of any thing malignant. All this pointed to the existence of a benign growth which was the source of the hemorrhages, but which, could it be safely removed, would not, in all probability, return. The large size of the growth and its soft character as determined by rectal examination appeared to be the only sign pointing to any thing else. Yielding to Dr. Van Buren's clinical experience and declining an operation was proved by the autopsy to have been eminently judicious. Fig. i shows the exact appearance of three of the fragments obtained during life, and none of the others differed essentially from these. After the autopsy a portion of the tumor was sent to Dr. Hailes, who, with the aid of his freezing microtome, got sections through the whole length of these papillæ and the wall of the bladder from which they sprang, shown in fig. iii, and which could probably scarcely have been obtained in any other way. It is easily seen that the infiltration of malignant elements extends only a short distance up into the papillæ, and explains how the portions we obtained during life gave false evidence of the character of the growth as a whole. Fig. iv shows a portion of the infiltration still more highly magnified.

Another study of great interest in connection with this case was that of the urine. During the last four months and a half of Mr. J.'s life the average amount of fluid passed from his bladder each day was in round numbers 90 oz., the specific gravity of which stood very steadily at



FIG. 3.

Is a section through the mucous membrane of the bladder, showing two large dendritic papillary vegetations implanted upon a carcinomatous base, and resemble benign papilloma, except near the base.

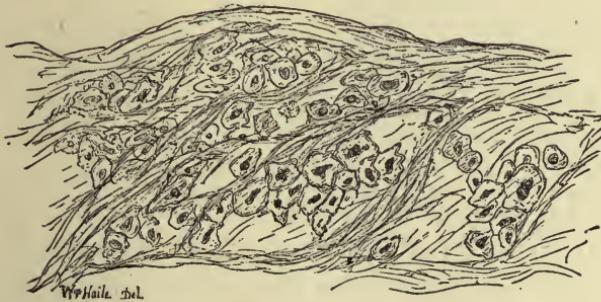


FIG. 4.

Encephaloid carcinoma, showing well-marked alveoli, with the large epithelial cells of encephaloid disease. This field was taken from near the base of one of the papillæ shown in fig. iii.

1010. After boiling, and the addition of nitric acid, the precipitated albumen occupied, after standing four hours, from one-third to three-fifths of the bulk of fluid used. Repeated and careful examination failed to show a single cast at any time. The urea varied from one-half to six-tenths of one per cent., which, considering the amount of fluid passed, was just about normal. There was not a single rational sign of any form of Bright's disease, and Dr. Van Buren's reply, when questioned on this point, was: "I will stake my professional reputation that the patient has no organic disease of the kidney."

I have no doubt that a large part of the fluid passed from the bladder was albuminous liquid exuded through the thin walls of the infinite number of villi constituting the exposed portion of this growth. The examination of a small portion of the fluid taken from the distended ureters at the autopsy supported this view, for it contained only a distinct trace of albumen and no more—less, in fact, than might have been expected considering the mode of death.

An interesting point in connection with the mechanical distention of the ureters and pelves of both kidneys, was that the same condition was found, on microscopical examination of the kidneys, to exist in the tubuli uriniferi, though the organs appeared to the naked eye to be healthy.

The persistent existence, then, of a large amount of albumen in the urine, without casts, without diminution of the total amount of urea excreted, and without rational signs of Bright's disease, might in other cases prove a valuable early diagnostic sign.

In the treatment of the cystitis, which was at first a very painful complication in this case, lactic acid gave more relief than any other drug, and its value has been attested in many other cases in my experience. For the suggestion

as to its use I am indebted to an article in the *Buffalo Medical and Surgical Journal*, for Feb., 1879, by Mr. Theodore Deecke.

The entire freedom from disease on the part of all the other organs of the body, and the fact that the recto-vesical septum was not invaded, were noteworthy.

I imagine that the mode of death by occlusion of the orifices of the ureters and the production of uræmic coma must be rare, for I do not find it recorded in the history of any case that has come under my notice. The probability of its having occurred was, in this instance, foreseen about a week before death.

The diagnosis from the *post-mortem* examination of the bladder, ureters, and kidneys would be carcinomatous papilloma of the mucous membrane and walls of the bladder, with hydronephrosis of both ureters and kidneys. There was also a marked dilatation of the straight and convoluted tubules in the medullary and cortical portions of the kidneys.

The soft papillomatous growth resembled cauliflower excrescences, and involved fully one half of the internal surface of the bladder, extending from the neck to the *bas fond* and sides.

This report, the micsoscopical examinations, and drawings were all kindly made by Dr. William Hailes, Jr.

*Literature.*—The treatises on general surgery all devote a few pages, more or less, to the consideration of tumors of the bladder. They all agree in dividing them into benign and malignant.

The names bestowed upon the varieties of the former class differ very considerably, and are founded rather on the results of microscopical examination after death, or in a few cases after removal, than on any possible clinical diagnosis. It is admitted on all hands that the growth

variously known as papilloma, villous papilloma, villous tumor, and vascular tumor, is the most common, and that simple fibroids, mucous polypi, and fibro-myomata occasionally occur.

Of the malignant growths encephaloid, scirrhus, and epithelioma are found, though authors do not agree as to their relative frequency.

As to the value of microscopical examination of portions of a tumor of the bladder obtained in the urine during life authors differ. Sir Henry Thompson, in his Clinical Lectures,<sup>1</sup> is cautious, basing his lack of confidence on the fact that there is no characteristic "cancer-cell," so-called. He speaks, however, of having "detected under the microscope the peculiar structure which the processes of the villous tumor present to the eye."

Harrison<sup>2</sup> says "the microscope is most valuable in detecting small portions of genuine villous growth," and the majority of authors speak as if finding such villi, free from any evidence of malignancy, would settle the diagnosis. In the case now reported a number of such villi were found, and yet the autopsy showed that any effort to remove the tumor by operation could have resulted in no good, in consequence of its nature.

Harrison says, at p. 361, that "tumors presenting some of the appearances of these villous growths, but of a malignant nature, are occasionally met with." But the distinguishing points which he mentions, such as induration, tendency to involve neighboring organs, implication of glands, and cachexia, were all absent in this case.

Gross<sup>3</sup> says that papillary fibroma is frequently confounded with villous carcinoma, but we do not find that he

<sup>1</sup> London, A. & A. Churchill, 1876, pp. 351.

<sup>2</sup> Surgical Disorders of the Urinary Organs, Wood & Co., 1881, p. 358.

<sup>3</sup> Diseases, Injuries, and Malformations of the Urinary Bladder, Henry C. Lea, 1876, p. 136.

any where mentions the possibility of what occurred in this case—that the base of the tumor proved to be a carcinomatous mass, originating in the bladder, not extending to any other organ, and covered all over with a growth of benign villi.

Gant<sup>1</sup> says nothing on this point.

Mr. Coulson,<sup>2</sup> in his admirably complete book, says “there can be no doubt that malignant growths occur, springing from mucous membranes, and having their surface covered with shaggy projections.” Mr. T. Holmes<sup>3</sup> says, in two lines, “a cancerous tumor may also sometimes be covered by villous processes of mucous membrane.”

The lesson to be learned from this is that Sir Henry Thompson’s caution to his students is eminently sound: “Most valuable as is the microscope in this great class of maladies, ranking next and very near to the sound itself, never let it obscure for you those broad features of the case which are to be determined by the unassisted eye and touch.”

On the subject of the persistent albuminuria which was present in this case the general feeling of the profession is well laid down by Dickinson.<sup>4</sup> He says that albuminuria may be artificially produced by the ingestion of a large quantity of highly albuminous food; perhaps by a rapid absorption of a large amount of serous fluid from the pleura, and some exceptional forms of hepatic disease. “With this exception, it may be stated, as a rule, that when the urine contains albumen the kidneys are abnormal either in circulation or in structure.”

Yet here is a case in which we think that a great and per-

<sup>1</sup> Frederick James Gant, Diseases of the Bladder, Lindsay & Blakiston, 1876.

<sup>2</sup> Walter J. Coulson, Diseases of the Bladder and Prostate Gland, Wood & Co., 1881, p. 116.

<sup>3</sup> Treatise on Surgery, Henry C. Lea, 1876, p. 770.

<sup>4</sup> Treatise on Albuminuria, Wood & Co., 1881, p. II.

sistent albuminuria did not depend upon any form of kidney trouble.

The only reference we have been able to find to this point is in Mr. Coulson's book.<sup>1</sup> He quotes Dr. Ultzmann as pointing out in an article, *Ueber Hæmaturie*, in the *Wiener Klinik*, May, 1878, the fact that "in cases of villous tumor the urine always contains more albumen than corresponds to the blood or pus in the sediment." He accounts for it in the same way that we have in this case, though Mr. Coulson's book and this reference came under our notice long after the first part of this article was written. Dr. Ultzmann further points out the fact that fibrine sometimes appears in the urine under the same circumstances and from the same cause.

Gross mentions the possibility of death from uræmia from occlusion of the orifices of the ureters in the pelvis of the kidney. But we have not happened to notice the occlusion of the ureters at their entrance into the bladder.

The following histories of cases, more or less similar, related in medical journals, have come under my notice:

1. *American Journal Med. Sci.*, vol. 16, 1835, page 522.
2.       "       "       "     7, 1844,   "   122.
3.       "       "       Oct., 1874,   "   561.
4.       "       "       Oct., 1879,   "   579.
5.       "       "       July, 1880,   "   233.
6. *Dublin Journal Med. Sci.*, vol. 16,       "   333.
7. *Medico-Chir. Review*,       "   19, 1831,   "   453.
8. *London Lancet*       "   1, 1849,   "   43.
9.       "       "       1, 1850,   "   188.
10.      "       "       1, 1854,   "   212.
11.      "       "       2, 1880,   "   978.
12. *British Med. Journal*,       "   1, 1879,   "   854.
13. *Med. Times and Gazette*,       "   1, 1879,   "   710,
14. *N. Y. Med. Record*,       "   8, 1873,   "   342.
15.      "       "       14, 1878,   "   395.
16.      "       "       16, 1879,   "   82.
17. *N. Y. Med. Journal*,       "   20, 1874,   "   62.
18.      "       "       21, 1875,   "   503.

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<sup>1</sup> *Op. cit.*, p. 188.

19. *N. Y. Med. Journal*, vol. 23, 1876, page 299.
20. " " 27, 1878, " 166.
21. " " 28, 1878, " 629.
22. *Archives Générales de Méd.*, " 1, 1876, " 489.
23. " " " 1, 1877, " 233.
24. *Medical News and Abstract*, 1881, " 854.

## THE ACTION OF VERATRIA ON THE VENTRICLE OF THE FROG'S HEART.

BY SYDNEY RINGER, M. D.,

PROFESSOR OF MEDICINE AT UNIVERSITY COLLEGE, LONDON.

IN these experiments I used Roy's tonometer. In most cases I employed the entire ventricle; in some only the lower two thirds, the portion free from nervous ganglia.

I used dried bullock's blood dissolved in water and diluted with saline in the proportion of one part blood to two saline.

In some experiments the ventricle beat spontaneously; in others contractions occurred only on the application of an induction shock.

A number or the sign + is given in the charts, to indicate the application of an induction shock. The number indicates the place at which the coil stood. The series of rises under either a number or + are due to a single induction shock.

The traces read from left to right.

Since temperature modifies the action of veratria on the ventricle of the frog's heart, I shall describe, first, my results with a high temperature, between 80° and 90° F.; then with a moderate temperature, between 60° and 70° F.; and, lastly, with a low temperature of about 40° F.

*Effect of veratria with a high temperature.*

The effect of veratria is in many respects best manifested when the heart does not beat spontaneously, but only on excitation, as shown in trace i, taken when the temperature of the room was close on 90° F.

Veratria first prolongs the duration of the systole, so that the summit of the amplitude becomes rounded; and this is still better seen in traces taken in a cooler room, or when the blood is cooled by ice.

Next, each excitation produces two contractions, but the interval is so short that the diastole is incomplete and the two traces become partially blended. In many instances, after the first contraction, the heart bulges at some point, and the diastole does not affect the whole ventricle; there is, indeed, incoördinate action of the ventricle. Each excitation soon induces a still more prolonged effect, and we get three, four, or more contractions produced in the manner just now described. Some portion only of the ventricle dilates, sometimes a small portion, sometimes a larger, and then re-contracts; and the degree of the trace-fall depends on the size of the bulging. In a few minutes each excitation produces a prolonged contraction, or series of contractions, lasting from 30 to 120 seconds; then after the excitation the ventricle suddenly contracts and the trace suddenly rises, then falls, sometimes considerably, at other times slightly, and the trace shows the ventricle is beating very incoördinately; but as the effect of the excitation persists this incoördination becomes much less, and in some cases even disappears. After a larger dose the trace suddenly rises, then falls slightly, and remains at nearly the same height as a straight line for 30 or 40 seconds; then the line becomes wavy. Though the line appears straight, a lens will often show that it is wavy, but in some cases the

line is quite straight. After it has become wavy for a variable time it slowly falls toward its original position on the base line. When the trace forms a straight line or the undulations are very small a considerable movement is visible in the ventricle, consisting of rapid, small, peristaltic waves of dilatation and contraction, running generally from base to apex.

With a larger dose (trace i, *C*), after remaining at much the same height for many seconds the trace slowly falls toward the base line, giving a trace much like the fall in a tetanized skeletal muscle.

The duration of the effect depends on the interval between the excitations. If, as soon as the effect of one excitation is over another excitation is applied, the duration of the effect of the second excitation is much diminished. In fact, the duration of the effect is in proportion to the duration of the diastolic pause. (Trace i, *B*.)

The irregularities in the trace are best studied by examining the ventricle. One sees extreme incoördination, especially as regards dilatation. A portion dilates when the rest remains contracted, hence pouching occurs, and some pouchings contract before others.

In these and other traces that I have taken it is noticeable that although the form of irregularity varies in different experiments, yet in each experiment the irregularity observes much the same type (see traces ii and iii).

At the end of a prolonged effect of an excitation the veratria irregularity often becomes less, and a ventricle at first very incoördinate at last beats nearly or quite coördinately (see trace i, *B*).

The sustained contraction, much longer after a large dose, which occurs for a brief interval after each contraction, is not due, as some hold, to an altered physical condition, lessening the elasticity of the ventricle, but

is due to muscular contraction—is, in fact, a prolonged spasm.

1. Either spontaneously or after an excitation the ventricle at first remains contracted for a variable time, then very incoördinate contractions ensue, and at last the contractions become quite coördinate (see traces i, iii, iv), and the ventricle stops in diastole. This series of events could not be explained were the elasticity of the ventricle alone affected.

2. In some cases after a contraction the ventricle relaxes greatly, but not completely, and remains in this semi-relaxed condition, and a strong vermicular action sets in; after a time the ventricle suddenly and completely relaxes with cessation of the vermicular action. This course of events cannot be explained on the supposition that veratria simply lessens elasticity (see trace vii, *F* and *G*).

3. Pouching, now of one part and next of another, cannot be explained on the supposition that the changes in the ventricle are due to lessened elasticity.

We see, then, that veratria induces, first, a change in the ventricle, so that each contraction is prolonged, and the amplitude of the trace is broader and its summit rounder; next, a single excitation, instead of one response, induces two contractions (see trace i, *A.* iii, *B.*); in other words, each excitation produces an increased expenditure of force.

As the effect of veratria progresses (more speedily after a large dose), each excitation produces a prolonged series of contractions, lasting thirty or more seconds, and the ventricular action becomes extremely incoördinate. Veratria, then, increases not only the expenditure of force set free by each excitation, but it lessens or destroys the conditions which make the action of the ventricle coördinate. These effects veratria produces by its direct action on the muscu-

lar tissue, since they were obtained with the lower two thirds of the ventricle, a part free from nervous ganglia.

Temperature strikingly influences the action of veratria on the ventricle. A small dose greatly affects the ventricle at a moderate or high temperature, but at a low temperature produces no effect; and at a low temperature a large dose, whilst greatly increasing the duration of the contraction (increased expenditure of force), only slightly incoördinates the action. It is probable, therefore, that the temperature of fever modifies the influence of remedies, and that veratria affects the heart more powerfully in fever than in health.

The incoördinate action is produced mainly, if not exclusively, by incoördinate dilatation; one portion dilates and forms a pouch, whilst the rest remains contracted. After each excitation there is, first, a coördinate contraction, but then some portions *remain contracted longer than others*, so that irregular dilatation ensues.

Veratria, therefore, affects the heart much as it affects the skeletal muscles. With a skeletal muscle veratria enormously prolongs the contraction and yet longer its relaxation. When the relaxation is complete well-marked fibrillary contractions set in, lasting many seconds, but produce no shortening of the muscles. These effects are not due to the influence of veratria on the nervous system, for they occur, I find, when the motor nerves are completely paralyzed by curare. With the skeletal muscles, then, veratria produces an increased expenditure of force, with incoördinate action of the fibrillæ.

In the muscular tissue (as, indeed, in nervous and secretory tissues) we have not only the fuel whose combustion supplies the force to the tissue, but we have, besides, a mechanism to convert the liberated force into muscular motion, nervous action, or secretion. There must also be a mechan-

ism to regulate the amount of energy developed, and to co-ordinate the action of muscular and nervous tissues. A poison may affect the combustibility of the fuel, or the mechanisms, or both, and so paralyze a structure. Veratria, I venture to suggest, disorders the machinery, and so causes at first an increased expenditure of energy, then incoördinate action, and at last complete paralysis.

The action of veratria on muscle is in many respects comparable with the effect of strychnia on the spinal cord. Instead of a coördinated response of short duration, an excitation calls forth from a strychnized cord a prolonged and incoördinated action. The slightest excitation spreads throughout the cord and causes a general evolution of force, lasting a considerable time.

Like veratria on muscle, strychnia weakens or destroys those conditions in the cord which determine the amount of force evoked by an excitation; hence the prolonged effect of an excitation on the strychnized cord. At the same time, strychnia destroys the mechanism which coördinates action, so that the response is incoördinate.

Continued strong faradization affects the heart much like veratria. Under the influence of a strong interrupted current the ventricle becomes contracted, then small bulgings occur, sometimes at one portion and then at another, and these bulgings immediately contract again. After a time the bulgings increase in size, and then both systole and diastole become most irregular.

If a poison can cause incoördinate action by its direct action on the muscular tissue, and independently of its action on the nervous tissue, it is probable that disease of the muscular tissue may also cause muscular incoördination. The fibrillary twitchings in progressive muscular atrophy, which are so similar to the fibrillary movements after poisoning by veratria, are probably due to changes affecting

the muscular machinery; also some cases of irregular action of the heart are probably due to disease of the muscular substance, and not of the cardiac nervous apparatus.

Further, the lower two thirds of the ventricle, the portion free from nerves, contracts coördinately, spontaneously, or when excited; and this implies a coördinating mechanism which disease may destroy, and so produce incoördination.

Atropia in some respects antagonizes the action of veratria, for atropia stops or greatly lessens the irregularities, though the heart's action becomes much weaker.

Traces viii, viii (*a*), and ix, show the antagonizing effect of atropia.

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#### *Explanation of Plates.*

##### *Effect of veratria with a high temperature.*

Trace i.—With the whole ventricle, which did not beat spontaneously. *A*, Effect of 5 minims of 1-in-500 solution of veratria added at the point indicated by the arrow. *B*, Nine minutes later. *C*, After an additional 20 minims of the solution.

Trace ii.—With the whole ventricle, which did not beat spontaneously. Well shows the effect of a single excitation, after a larger dose of poison. Here there are no undulations in the early part of trace following the excitation, even with the aid of a lens. After 40 minims of 1-in-500 solution.

##### *Effect of veratria with a temperature between 60° and 70° Fah.*

Trace iii was obtained with the lower two thirds of the ventricle, a portion of the heart free from nervous ganglia. It is a good instance of the effect of veratria when the temperature of the blood is between 60° and 70°. As the effects from this segment of the ventricle are the same as those which occur when the entire ventricle is used, it is evident that the veratria manifests its action on the muscular tissue.

As with a higher temperature, so veratria with a moderate temperature first prolongs each contraction. Then each excitation induces two contractions; but as the second contraction does not begin till the completion of the first, they remain dis-

tinct, instead of being blended, as happens when the temperature is higher. Next the trace becomes affected, as with higher temperatures, and need not be more fully described here. This portion of the ventricle did not beat spontaneously. *A*, Before veratria. *B*, After 5 minims of 1-in-500 solution. The + indicates the excitation. It is seen that each excitation induces two contractions. *C*, After a larger dose. *D*, After a still larger dose.

Trace iv.—With the whole ventricle. Shows the gradual development of the veratria effect. *A*, Before veratria. *B*, Five minims of 1-in-500 solution added at the point indicated by the arrow. *C*, Eighteen minutes after veratria. *D*, About forty minutes after veratria.

Trace v.—Shows the effect produced by a large dose at a moderate or high temperature. Each rise becomes broader, then coalesces with its fellow ; that is, one contraction begins before the previous one ends, and at last forms nearly a straight line raised high above the base line.

This trace was taken with the whole ventricle. At the arrow ten minims of 1-per-cent. solution of veratria was added.

I now give the effect of veratria, with blood cooled, by a freezing mixture, down to  $8^{\circ}$  or  $10^{\circ}$  Fah.

Trace vi.—With the whole ventricle. *A*, Before veratria and after the blood was cooled to about  $8^{\circ}$  to  $10^{\circ}$  Fah. *B*, About nine minutes after 15 minims of 1-per-cent. solution of veratria. *C*, About eighteen minutes after veratria. *D*, Later effect.

Sometimes even at a low temperature veratria in large doses incoördinates the ventricle, but in a far less degree than when the temperature is high. This is exemplified in trace vii, which also shows the alternating effect first of cold, then of heat, and then again of cold.

Trace vii.—With the whole ventricle. *A*, Temperature of blood  $63^{\circ}$  F. *B*, Temperature of blood  $45^{\circ}$  to  $46^{\circ}$  F. *C*, Two minutes after 15 minims of 1-per-cent. veratria solution. Temperature of blood  $43^{\circ}$  F. At first there occurred decided irregularity. *D*, About eight minutes after veratria. *E*, eighteen minutes after veratria. Temperature of blood  $40^{\circ}$  F. I then removed the ice-and-salt mixture. *F*, Thirty-six minutes after veratria. Temperature of blood  $64^{\circ}$  F. Here after the rise a decided fall took place, and for some time, eight to ten seconds, the trace remained at the same point, and then suddenly fell. Whilst it remained horizontal, and before the final sudden fall, very distinct frequent peristaltic action occurred, generally proceed-

ing from base to apex. After the final fall this ceased. *G*, forty-five minutes after veratria, temperature of blood  $81^{\circ}$  F. Here the tracing is the same as in the last, except that the primary fall was greater and the final fall very slight. I then raised the temperature of the blood to  $86^{\circ}$  F. and the heart's action grew very weak and infrequent. *H*, fifty-four minutes after veratria, blood  $86^{\circ}$  F. I then again applied the freezing mixture. *I*, Sixty-three minutes after veratria, blood  $59^{\circ}$  F. Here, again, after the primary fall and before the final fall, well-marked frequent peristalsis occurred. *K*, Seventy-four minutes after veratria, blood  $41^{\circ}$  F.

*Tracings showing the antagonizing effect of atropia.*

Trace viii.—With the whole ventricle, temperature of blood about  $90^{\circ}$  Fah. *A*, Effect of 5 minims of 1-in-500 veratria solution. At the point indicated by the arrow 10 minims of 1-per-cent. solution of sulphate of atropia were added. *B*, At the arrow other 10 minims of 1-per-cent. atropia solution added.

Trace viii (*a*). Temperature between  $60^{\circ}$  and  $70^{\circ}$  Fah. *A*, Before veratria. *B*, Effect of 5 and 10 minims of 1-in-500 solution of veratria. *C*, Shows the effect of atropia; at the arrow 20 minims of 1-per-cent. solution of sulphate of atropia added. *D*, Ten minutes later.

Trace ix.—With the whole ventricle, temperature of blood  $66^{\circ}$  Fah. *A*, Before veratria. *B*, After 30 minims of 1-in-500 solution of veratria. The notch in this trace was due to a broad peristaltic wave of dilatation and contraction, and shows that when this is large enough it affects the trace; at other times, when smaller, as previous traces show, the trace is unaffected. *C*, After 70 minims. *D*, After 10 minims of 1-per-cent. solution of sulphate of atropia. *E*, About a minute later.

## DRAINAGE IN THE REMOVAL OF SUB-MUCOUS FIBROIDS.

BY W. H. BAKER, M.D.

INSTRUCTOR IN GYNECOLOGY, HARVARD UNIVERSITY.

THE importance of thorough systems of drainage, both from hygienic and surgical standpoints, has received much consideration of late, and has given an increased stimulus alike to the sanitary engineer and the surgeon, in furnishing efficient means for the removal of deleterious material. The surgeon sees the most immediately serious results from delay in the establishment of good drainage. He therefore arranges for the escape of effete matter even in advance of its formation, and is greatly concerned at any interference with the thorough working of the means thus provided.

Although the adoption of Lister's antiseptic principles has, to a certain extent, diminished the necessities of drainage, either by lessening the amount of obnoxious fluids, or by absolutely preventing processes resulting in their formation, yet the careful surgeon must always prepare a way of escape for such objectionable material, let its cause be what it will. The amount of tissue destroyed and left to be taken care of in the reparative process may be great; some defect may exist in the perfect carrying out of the antiseptic treatment; and even when there is no defect, the difficulties in its way are often serious in those parts

having a natural outlet which must, in a measure, be kept patent. Whether these causes are present at the time of the primary operation, or arise subsequently to it, the surgeon knows that they are best dealt with at the time of the operation, and that his patient may thus oftentimes be spared the ordeal of a second surgical interference.

The necessity of providing drainage in the removal of submucous fibroids was first presented to me in a case reported by my honored instructor, Dr. J. Marie Sims, in his valuable article on "Intra-Uterine Fibroids," published in the *New York Medical Journal* for April, 1874, being case No. 6 of those reported. The case was indeed most grave, the patient being blanched and anaemic to the last degree; while the tumor, fibro-cystic in character, reached above the umbilicus. The os uteri was fully dilated; and the vagina was filled with the growth, which, in that part, was nearly sloughing. It was the first operation of the kind I had ever witnessed; and I can never forget the boldness and brilliancy of each step of its performance, nor the profound respect and admiration I felt for the operator.

It was my fortune to have the subsequent treatment of the case, which consisted locally of the frequent irrigation of the cavity of the uterus with carbolized water for several days after the uterine and vaginal tampons were removed. Suddenly on the nineteenth day, the intra-uterine injections having been discontinued, the patient had a chill followed by intense fever, the temperature being 106° and the pulse 160. It was then found that the uterine structure was so softened as to allow a flexion of the organ upon itself at the os internum, thereby occasioning an obstruction to the free flow of pus from the uterine cavity. I succeeded in adjusting a block-tin tube, three inches in length, in the cervical and uterine cavities, which was left in place as long as any discharge continued. It acted most satisfactorily;

the patient had no more chills or other indications of septic absorption, and made a good recovery. The great benefit derived from the drainage tube in this case determined me to try it in necessary instances subsequently, substituting a hard-rubber tube for that of block tin, and making its application at the time of the operation instead of waiting until dangerous symptoms presented themselves. I now wish to present seven cases in illustration of the advantages to be derived from this plan of treatment, as well as some facts regarding the form and adjustment of the tube, taught me by my experience.

CASE 1.—Mrs. H., forty-one years of age, had been married five years, and had had one child then four years old, and no abortions. The menstruation, which, up to the birth of her child, had been perfectly regular and normal, at that time began to increase both in amount and duration, until the flow would continue active for nine or ten days and oftentimes drag on several days longer, causing exceeding exhaustion. During these days the patient was obliged to remain in bed, using the various ordinary means to control the hemorrhage. For some months she had taken large doses of Metcalf's fluid ext. of ergot, amounting more latterly to nearly  $\frac{1}{2}$  ounce three or four times daily, but had obtained very little control of the hemorrhage, and little if any contractile effect upon the uterus. The depth of the uterine cavity was fully five inches. Among many minor troublesome symptoms complained of, was frequent micturition.

For the purpose of exploring the interior of the uterus and removing, if possible, the sub-mucous fibroid which had previously been diagnosticated, a sponge tent was inserted into the uterine canal March 28, 1878. After sixteen hours this was removed, and seven laminaria tents of large size substituted. Seven hours were allowed for dilatation by these last tents, and, ether having been administered, assisted by Dr. J. P. Reynolds and Drs. E. G. Cutler and C. M. Green, I was able to remove the fibroid, which was of the size of a large goose egg and attached over fully one third of its surface. The enucleation was accomplished by keeping up a constant traction with the volsellum forceps, cutting through the capsule at the lowest point of attachment, and separa-

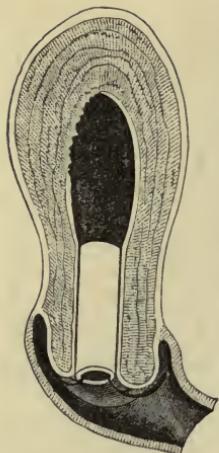


FIG. I.

ting the growth from the uterus with the finger. The stronger bands of attachment were cut with the scissors, the points of which were guided and guarded by the finger. The growth was entirely within the body of the uterus, not having pressed sufficiently upon the internal os to occasion the slightest dilatation or softening there. As soon as the tumor was removed and the portions of capsule which were free had been trimmed off, a hard-rubber drainage tube,  $2\frac{1}{4}$  inches long, with an internal diameter of  $\frac{3}{4}$  of an inch, was inserted in the canal of the uterus, its inner end reaching through the internal os, and its external end coming flush with the external os, where it was retained in its position by two silver sutures

passed through the anterior and posterior lips of the cervix respectively, care being taken not to twist them too tightly lest they might cut themselves out of the tissues. Two small holes had previously been made in the tube for the reception of the sutures (fig. 1). The cavity of the uterus was then thoroughly syringed with hot water, and Churchill's tinct. of iodine applied to it. A tent of styptic-iron cotton was placed in the drainage tube, and the vagina was tamponed with ordinary cotton. Not more than one ounce of blood was lost during the operation. With the exception of some irritation of the throat occasioned by the ether, the patient felt comparatively comfortable after the operation.

The tampon was removed March 30th, and the uterus thoroughly washed out with carbolized water three or four times daily for a week afterward, by placing the long nozzle of the fountain syringe directly into the drainage tube. On the third and fourth days the temperature reached  $101^{\circ}$  and the pulse 100, the highest points noted. By the fifth day they both became normal and afterward remained so. From the third to the sixth days there was more or less muco-purulent discharge, with occasional shreds not only washed away with the injections but also discharged upon the napkins between times. By the eighth day all this had been entirely absent for two days, and the pulse and temperature had been normal for three days; the drainage tube was therefore then removed and vaginal injections substituted. On the fourteenth day, the depth of the uterine cavity was three

inches. The patient was that day allowed to sit up, and the next day to be about her room. The recovery was uninterrupted. A year afterward the uterus was but two and three quarter inches in depth, and the patient reported the menstruation as having been perfectly normal since the operation.

CASE 2.—Mrs. S. was admitted to the Free Hospital for Women, March 17, 1878. She gave every evidence of some terrible drain upon her. I have seldom seen a person in so bloodless a condition. She was 32 years of age, and had been married twice, having had two children by the first husband, but no abortions. The menstruation, which began when she was 14 years of age, was regular and normal up to the death of her first husband, seven years before. At that time she noticed an irregularity in its recurrence, being a shortening of the intermenstrual period. The flow was also very pale in color. Three months before her admission to the hospital she had profuse menorrhagia, since which time she had kept her bed. Previous to this hemorrhage, there had been no undue flowing, but rather a diminished amount, sufficient sometimes to soil only one napkin. She had noticed a swelling in the hypogastrium for seven months, which had not, however, perceptibly enlarged during this time. She had complained of considerable leucorrhœal discharge before taking to her bed. Her appetite and digestion were good. A physical examination was made, and the case diagnosticated as one of submucous fibroid of the uterus. Being in hospital, where any hemorrhage could be controlled, should it recur, it was thought best to bend all efforts to gaining a certain amount of strength preparatory to the operation for the removal of the growth. She was therefore ordered a generous diet, cod-liver oil, and iron. This treatment was persisted in for two months, at the end of which time she began to show some color in her cheeks and hands, and to be about the room. During these eight weeks there were two slight attacks of flowing, which were readily controlled by the vaginal tampon.

May 24th, the patient gave evidence of being not quite clear mentally; and the temperature, previously normal, rose to 102.5°. At the same time there appeared an offensive and purulent discharge from the vagina. There was every indication that the tumor had begun to slough. Its immediate removal was determined upon, and one large sponge tent was introduced. Sixteen grains of quinine were also given. In twelve hours the sponge tent was changed for six laminaria tents, by which time the patient was quite delirious.

May 25th. The patient was etherized by Dr. C. M. Green, the house-surgeon, and, the tents being removed, full dilatation was found to exist. The tumor was firmly adherent to the left side and anterior part of the uterus, from the fundus to the os internum, and was sloughing superficially. The removal was similar to that in Case 1, except that, as the tumor was much larger in size, it was necessary to remove it in two pieces. Several large shreds were also cut away. There was almost no loss of blood. The drainage tube and vaginal tampon were adjusted as in the first case. On account of great weakness of the pulse, subcutaneous injections of brandy, and also quinine, were given during the operation, and for several hours following it. Within thirteen hours after the removal of the tumor, the temperature had fallen to  $98.9^{\circ}$ , while the pulse was 96 and fairly strong. The next day, however, the temperature again began to rise; the vaginal tampon was removed, and vaginal injections of carbolized water instituted and continued each four hours.

May 27th. Large flakes of degenerated material and shreds were washed away, the nozzle of the syringe being placed in the drainage tube. By June 3rd these had entirely disappeared, the water returning perfectly clear and inodorous. The tube was removed on the eighth day after its adjustment, and there was no perceptible discharge subsequently. The temperature ranged most of the time between  $99^{\circ}$  and  $100^{\circ}$ , and the pulse between 90 and 100. It was not necessary to use any opiates, there being no special pain complained of. There was more or less low muttering delirium, beginning the day before the operation and continuing nearly to the time of her death, June 14th. Five days before death, she was unable to talk intelligently, and could not protrude the tongue; there was also diminished sensation and partial paralysis of motion in the right cheek, right arm, and right leg, as well as considerable oedema of the arm and leg. No reaction was obtained from the right pupil. There was incontinence of urine. Two days later, although the swelling of arm and leg had increased, yet in the arm the power of motion was partially recovered. The day before death there was a diminution of the swelling of the right leg, and the femoral vein was felt to be hard and cord-like. Throughout nearly the whole time after the operation, the patient was sleeping, being easily aroused for her nourishment, which she was always ready for, taking milk, beef tea, and brandy freely.

The autopsy, performed by Dr. A. T. Cabot 18 hours after

death, showed a hemorrhage between the dura and pia mater over the right middle lobe, partially clotted and partially fluid-blood. The posterior branches of both middle cerebral arteries were closed by emboli, the resulting thrombi extending into the smaller branches completely closing them. About one third of each middle cerebral lobe, being the parts nourished by the occluded vessels, were softened. The lungs were oedematous. The vessels below the Fallopian tube in the right broad ligament were filled with clots partially decolorized. This thrombosis of the vessels involved the iliac vein, which was occupied by a thrombus extending from the region of Poupart's ligament to the junction of the vena cava. The proximal portion of this clot was light-colored and friable; the distal portion, dark red and recent. There was also a recent thrombus in the femoral vein of the right side. The uterus was four inches long, and from three quarters to one inch in thickness. The mucous membrane was somewhat thickened and injected on the left side, with a slightly projecting eminence from the left side of the wall, suppurating on its surface. The vessels of the body were dilated and thickened.

This case well illustrates one of the great dangers to which these cases are subjected. Reduced by excessive hemorrhage, with the blood in the most favorable condition for the formation of a clot, and with the greatly increased activity in the circulation of the uterine blood-vessels, death may occur, as the autopsy proved in this case, from emboli plugging some of the cerebral arteries. This danger, together with that from septic absorption from a sloughing fibroid, must be cautiously considered, and full importance ascribed to each, when we decide to delay our operative interference. In the case just cited, the autopsy showed involution to have begun, and the healing process in the uterus progressing favorably; and the result might not have been different had the operation been done earlier. Yet I can but feel that the patient might have had a better chance had the tumor been removed months before. The next case, although not one of uterine fibroid, yet illustrates so well not only the importance of drainage, but also one of

the difficulties which may occur, necessitating a change in the form of the tube used, that I have introduced it into this series.

CASE 3.—*Retained foetus and adherent placenta.* Mrs. B. was admitted to the Free Hospital for Women in the spring of 1878. She was 35 years of age, had been married 15 years, and had had two children, who were then 13 and 9 years old. There had been no abortions. There was nothing of special interest in her previous history. She was a farmer's wife, and had always enjoyed fair health. Menstruation appeared when she was 13 years of age, and had been regular and normal, with the exception that she had always suffered from dysmenorrhœa. The last menstruation had occurred in December, 1877. It was absent in January, and at the time for its recurrence in February she took cold, had a sick headache, and began to flow. This flow continued, with intervals of cessation, for six weeks, the patient being on the bed or sofa only part of the time, but losing much blood and suffering much from nausea. She did not think herself pregnant. The day following her admission to the hospital she was etherized; and, on removing the vaginal tampon placed by the house-surgeon, Dr. C. M. Green, at her own home, in order to control the hemorrhage and enable her to be moved to the hospital, a partially decomposed foetus, apparently about three months developed, with a portion of the placenta, was found lying in the vagina. After their removal, the interior of the uterus was explored and about one half of the placenta was found firmly adherent to the left side of the uterine wall. This tissue was not soft and friable, easily breaking down under the finger, or removed by forceps; but in its feel very much resembled sole leather, and seemed so intimately connected with the uterus as to form a part of it. It was a difficult and tedious matter to remove it. Holding it steadily with the volsellum forceps, and guiding the scissors over the point of the left forefinger, this very tough tissue was cut from the uterine wall, the thickness of which was from time to time determined by bi-manual touch to assure myself that this organ was not cut through. An intra-uterine douche of hot carbolized water was then given, and ordered to be repeated each six hours, and the drainage tube adjusted as in the first and second cases. She was also ordered 4 grains of the aqueous ext. of ergot by the rectum *ter die*. The second day after the operation the patient had a slight

chill; and the temperature, which had previously been  $101.4^{\circ}$ , at once rose to  $103.7^{\circ}$ , and the pulse to 140. On seeing the patient, I learned that there had been no foul discharge or shreds returned with the washings, which had been given by inserting the end of the vaginal nozzle of the syringe into the end of the drainage tube. I attempted to pass a double catheter through the latter, and found the os internum closed over its inner ends. The force of the uterine contractions had evidently expelled the tube from the cavity of the uterus; but it was unable to slide from the cervical canal, as its outer end was stitched to the lips of the cervix, and the force of these contractile efforts had stretched out the cervix so that it contained the whole tube in its canal. (Fig. 2.) Under these circumstances the drainage tube could, of course, accomplish nothing. It was therefore removed, and a double silver catheter of large size passed into the interior of the uterine cavity, and made fast to the external parts by strips of adhesive plaster. There was at once discharged through it fully an ounce of purulent and highly offensive fluid. The intra-uterine injections were continued each two hours for two days, then each three hours, and then each four hours, until, by the eighth day after the operation, all discharge having ceased, the drainage tube was removed altogether. There was no after chill or elevation of temperature; and the patient was considered quite convalescent, when she suddenly, from some undue exposure, developed a slight attack of pneumonia, which kept her in the hospital about four weeks longer than would otherwise have been necessary. At the time of her discharge, which was between seven and eight weeks from the date of admission, the uterus had quite recovered its normal size. Within the past year I have heard of the safe delivery of this patient with her third child.

To prevent a like accident in future I had the hard-rubber drainage tube made longer by having its inner end bevelled after the shape of an ordinary Ferguson speculum. (Fig. 3.)

CASE 4.—Miss G. first consulted me January 29, 1878, complaining of dysmenorrhœa and leucorrhœa. She was 36 years of age, and a teacher. Her family history and her own general

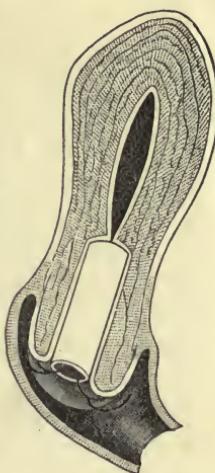


FIG. 2.



FIG. 3.

health were good. Menstruation appeared at 15 years of age and occurred regularly afterward. At the time of my first seeing the patient, the flow continued seven days, and was excessive, amounting to twelve thoroughly saturated napkins during the first two days, besides the discharge of many large and solid clots. The pain was described as most severe, being mainly dull, heavy, and grinding in character, coming on several hours after the appearance of the flow, and lasting perhaps two days, or during the time of its greatest excess, and then subsiding to reappear for an hour or two daily for four or five days more. The leucorrhœal discharge was thick and stringy, like the white of an egg, but not sufficient in amount to require protection. There was no difficulty in walking, no headache, and no trouble of the bladder or bowels. An almost constant backache was, however, complained of. A physical examination showed the body of the uterus anteflexed, with stenosis of the canal at the os internum which scarcely allowed the most delicate Emmet's probe to pass. A month later, a sponge tent was introduced, and a larger one substituted in twelve hours. Upon examination under ether six hours later still, it was found that the stricture had yielded but slightly to the tents, the touch, practised bimanually, conveying the impression of the existence of a small interstitial fibroid pressing on the os internum posteriorly. Two months after this examination, the cervix was divided bi-laterally, to the crown externally, and at the os internum to the depth of  $\frac{1}{4}$  of an inch. A conical glass plug was placed in the canal and the vagina tamponed. The patient made a good recovery from this operation, and reported to me again in January, 1879. Examination showed the calibre of the canal sufficient to admit readily a Peaslee's sound. But the dysmenorrhœa and menorrhagia both existed still, not having been relieved in the least degree by the previous treatment. She was now referred to the Free Hospital for Women, where she remained for five months, faithfully carrying out a general tonic and rest treatment, attempts being made to control the hemorrhage at the menstrual periods by the tampon—ergot not having any effect in accomplishing it. During this time she improved greatly in flesh and general strength, but the dysmenorrhœa rather increased. During the summer she was in charge of Dr. G. W. Porter, of Providence, who removed with the curette some hypertrophied glandular structure of the interior of the uterus without any previous dilatation. This seemed in a measure to relieve the menorrhagia, although the dysmenorrhœa was much less affected by the

treatment. On her readmission to the hospital in October, 1879, the uterus was still anteflexed; the depth of the uterine cavity was three inches, and the calibre of the canal quite normal.

After three successive menstrual periods, an attempt at dilatation was made by means of tents, in order more thoroughly to explore the internal cavity. The first two times the tents were pushed below the os internum by the violent contractions of the uterus, even though the vagina was firmly packed to keep them in place. The third time, however, the precaution was taken to pass a silver suture directly through the laminaria tent, which was of

large size, and also through the anterior and posterior lips of cervix. When this was removed, after twenty-four hours, it was found that the os internum was sufficiently dilated to force the forefinger through it. The touch then made the presence of a very small fibroid in the posterior uterine wall absolutely certain. It was so embedded that its inner surface projected but slightly into the uterine cavity. Its lower portion pressed against the internal os, and its size was that of an English walnut, somewhat flattened. Its removal was one of the most difficult operations I have ever performed. The os internum was not sufficiently open to admit the finger and at the same time allow the use of the necessary instruments for its removal. (Fig. 4.) Nor could it be further dilated on account of the situation of the fibroid, extending, as it did, somewhat around upon the side. Thomas' scoop was of no use, because it could not be introduced. By cutting into the tumor with scissors just within the os internum, I was soon able to detach enough of the growth from the uterus to allow me to seize it with a small pair of volsellum forceps; and, by continued traction and persistent efforts with scissors, finger, and at times one part of the handle of the scissors, I was at length able to remove the most of it, although it was the work of an hour and a quarter to accomplish it. The drainage tube, as modified, of a somewhat smaller circumference, was adjusted in the cavity of the uterus and stitched to the cervix externally; and the other vaginal dressings were applied as before. In the evening of the day of the operation, Feb. 23, 1880, the tem-

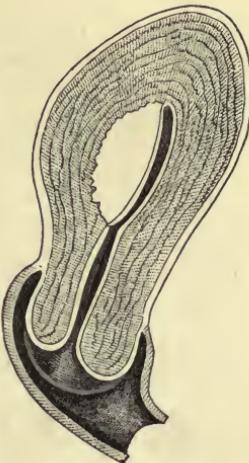


FIG. 4.

perature rose to  $100^{\circ}$ ; but the next morning it was normal, nor did it afterward rise above  $99^{\circ}$ . The pulse varied between 80 and 90 beats to the minute. The vaginal dressings were removed on the second day, and the intra-uterine injections continued after-

ward until the removal of the drainage tube on the tenth day, after all discharge had ceased. The patient was sitting up in two weeks from the date of the operation, and in two weeks more was discharged from the hospital cured. I have heard from her within two months, and there has been no severe dysmenorrhœa nor any abnormal amount of flow since the operation. In the prolonged care of this case, much credit is due my house-surgeons, Drs. F. W. Johnson and C. P. Strong.

CASE 5.—Miss H., æt. 36, consulted me May 8, 1880, on account of excessive menstruation, which had been gradually increasing for four years until she had begun to experience some feeling of debility from the continued drain. The time of the continuance of the flow had also lengthened from four days to a week and sometimes more. There had never been any dysmenorrhœa nor other symptoms that would indicate the nature of her trouble. She had always been very strong and well, never having to consider her own strength at all, whether for the continued tax of travelling and sight-seeing, or for the more tiresome duties of home and social life. Feeling assured herself that something must be wrong locally to account for the excessive flowing, she decided that it should be satisfactorily determined. For this purpose, ether was at once administered, and the body of the uterus found bi-manually

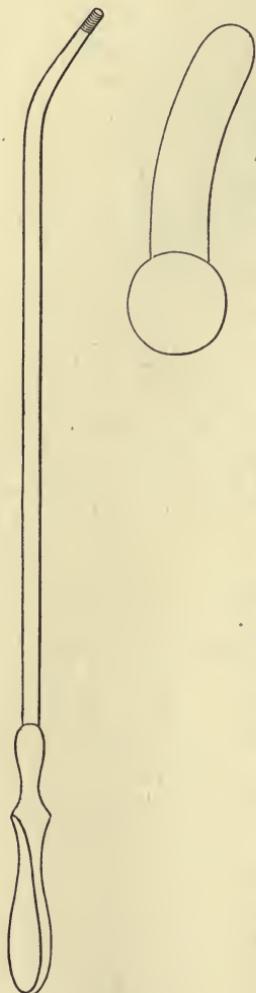


FIG. 5.

a little enlarged and anteflexed. By the passage of the probe, the uterine depth was found to be  $3\frac{1}{4}$  inches. Dilatation and exploration of the interior were advised; but, on account of her family changing from their winter residence in the city to their

country home, it was postponed until July 21st. I was assisted by Drs. F. H. Davenport and J. W. Elliot. The canal had been previously dilated with the sponge, followed by laminaria tents; but I found that in this case, as in the last, the tents had been expelled from the uterine cavity, and were lying in the cervical canal.

Rather than expose my patient to the greatly increased danger of at once inserting another set of tents, or yet to the delay of waiting until after another menstrual period, I decided to attempt rapid dilatation with a set of hard-rubber plugs which I had long previously had made for the purpose (fig. 5), graded in size from  $\frac{3}{8}$  to  $\frac{3}{4}$  of an inch in diameter, and three inches in length. They are supplied with steel handles screwed into the rubber plugs. It was the work of an hour to dilate sufficiently to allow the forefinger to pass into the uterine cavity. This time might have been very much shortened had I been willing to use either uterotome or scissors in dividing the cervix externally, internally, or both; or even had I used one of the more forcible instruments with divulsive blades. But with the dilators described and figured, I was able to avoid making any unnecessary denuded surface to be subsequently exposed to septic influences, and, at the same time, to distribute the force equally, simulating as nearly as possible the natural forces in the process of dilatation for the passage of a fibroid from the cavity of the uterus. This part of my work accomplished, fully another hour was needed to enucleate and remove a sub-mucous fibroid of the size of a large olive from the posterior and left lateral walls, situated pretty low down, and attached throughout three fourths of its extent. The opening through which I worked was exceedingly contracted. The tumor was rather friable, and was removed in shreds. The modified drainage tube of the smaller size, as in Case 4, was adjusted, and the after-treatment carried out in a similar manner with the most gratifying results. There was, however, in this case, much more abdominal tenderness on account of the prolonged efforts at dilatation; and for three days the temperature ranged between  $100^{\circ}$  and  $101.8^{\circ}$ , not rising afterward above  $99^{\circ}$ . There was at no time any indication of inflammatory action, and the drainage tube was removed on the eighth day. I have heard from the patient several times during the past year, and the menstruation is in every way perfectly normal.

CASE 6.—Mrs. S. was referred to me by Dr. Bacon, of Brockton, July 28, 1880. She was 42 years of age; had been married

25 years ; and had borne four children, the eldest of whom was 24 years old, and the youngest 11. Patient had never aborted. About a year before I first saw her, after recovering from an attack of diphtheria, she first noticed that she had pain in turning in bed, and also discovered an enlargement above the pubes.

Menstruation began at 12 years of age, and had been perfectly regular and normal up to the birth of her last child. After this event, the time of its continuance lengthened a day or two, and the amount increased, first from 2 or 3 napkins to 4 or 6, and still further increased gradually, until during the past two years the duration of the flow had become from 7 to 9 days, and the amount from 20 to 25 saturated napkins, besides the discharge of many large clots. She had also had two severe hemorrhages, five months and one month respectively, previous to consulting me. Much exertion of any kind would start the flow. She had suffered from a constant leucorrhœal discharge for five months. She was also troubled with a very frequent desire to micturate. She had been unable to do any work (dressmaking) for several months. A physical examination showed the uterus to be strongly anteverted, and in size about that of the gravid uterus at the third month. The probe passed into the uterine cavity 5 inches. The diagnosis of sub-mucous fibroid was made, and its removal advised.

Aug. 19th. The menstruation having been controlled by ergot, and having ceased four days before, the canal likewise having been dilated with sponge and laminaria tents, the patient was etherized, and, assisted by Drs. Davenport, Bacon, and Elliot, I enucleated the tumor in a manner similar to that, already described, except that the operation was much expedited by the use of Thomas' scoop. In size the tumor was equal to a large Florida orange. It was impossible to remove it as a whole ; and it was cut into several pieces before it was all taken away. It was attached over the whole extent of the anterior wall and fundus of the uterus ; and quite a large denuded surface was left after its removal. The drainage tube of the larger size was adjusted, as in the former cases, the uterus washed out, and the vagina tamponed. The after-treatment was exactly like that previously described. The tube was removed on the ninth day, all discharge having ceased. The temperature did not rise above 100°, and reached even that point only on the two days just following the operation. The convalescence of the patient was somewhat retarded by an attack of acute cystitis. The patient returned home September 10th.

She was seen several months afterward, and reported the menstruation to have been perfectly natural. I found the uterus to be likewise normal in size.

CASE 7.—Miss H., consulted me Aug. 6, 1880. She was 33 years old, very anaemic in appearance, and had complained for three years of a gradually increasing menstruation, at this time lasting one week, and amounting to over 30 saturated napkins. There were no clots discharged, nor was there any complaint of dysmenorrhœa. She apparently did not gain during the inter-menstrual periods the strength lost by the excessive flow, though she expressed herself as feeling very well, but tired between the menses. She suffered somewhat from leucorrhœa. Bi-manual manipulation revealed an anteverted uterus equal in size to the pregnant uterus at three and a half months. The forward displacement was so great that it was with great difficulty that I could pass the probe, which showed the uterine cavity to be four and a half inches deep. The diagnosis of sub-mucous fibroid was made ; and, as I was out of town for the summer, I advised the free use of ergot, beginning a week before the expected flow, and increasing its amount during the menstrual period. I also advised her taking iron between the menses. She entered my private hospital October 5, 1880, for the removal of the tumor. She had gained somewhat in strength during the eight weeks that had passed, the ergot serving partially to control the flow. The cervical canal was dilated with sponge and tupelo-wood tents. Oct. 6th, with the help of my two assistants, she was etherized, the tents removed, and the tumor found growing from the anterior and left lateral walls of the uterus. In size it was equal to a goose egg. Its enucleation was very much hastened by Thomas' scoop ; in fact, I had never before been so strongly impressed with its great value in these cases.

The greatest difficulty in the operation was, after enucleation, to remove it through so small a cervix and vagina without tearing them. It was finally accomplished, however, and the drainage tube adjusted as heretofore described. The after-treatment was also like that in the other cases ; and all went well until the fourth day, when the temperature began to rise and on the fifth day reached  $103.5^{\circ}$ . At my afternoon visit the patient was delirious. She had no chills, and there was a constant discharge from the vagina of disintegrating tissue and shreds. Intra-uterine injections had been kept up two and three times daily since the second day ; and there was apparently no cause connected with the drainage, for



FIG. 6.

the alarming state of the patient. Yet so sure was I of retention of decomposing fluids in the uterus, that she was put upon the table and Sims' speculum introduced, when it was found that the swelling of the lips of the cervix had quite overlapped the edges of the tube all around ; and, although the central opening of the tube was patent into the cavity of the uterus, yet any fluid collecting about the outside of the tube could not be discharged. (Fig. 6.) The two silver sutures which held the tube in place were therefore removed, and the tube taken out. At once there escaped a full half ounce of highly offensive fluid. An ordinary double silver catheter was then adjusted and

secured to the external parts. It was through this that the nurse subsequently gave the intra-uterine injections. The patient became restful, the delirium disappeared, and within twelve hours the temperature dropped to  $99^{\circ}$ . It was, however, necessary to retain the silver catheter for thirteen days, as there was some discharge up to that time. There was no after-interruption to recovery, and the patient was sent home the 29th day of the same month in which she entered the institution. She was examined on Dec. 18, 1880. The uterus was found normal in size, the probe passing  $2\frac{1}{2}$  inches. Felt bi-manually, it was also natural in size. The patient reported that since the operation the menstrual flow had continued for three days only, and amounted to but twelve napkins, eight only of these being saturated. She has been heard from several times since and continues to do well.

In order to avoid in future the difficulty which arose in this case, I then changed the shape of my drainage tube by rounding a flange on its outer end, and later, at the suggestion of Dr. Minot, the sides of the tube were perforated. The instrument as perfected is illustrated by fig. 7.

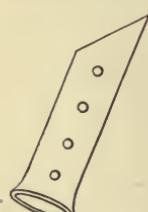


FIG. 7.

These cases, excluding the third, (which was introduced simply to show one of the difficulties I have found it necessary to overcome in perfecting the drainage tube) have

several features in common, among which may be mentioned :

All the tumors were myo-fibromata.

All were attached to the uterus over the greater part of one half of their surface.

All were in the body of the uterus, the os internum remaining undilated by any pressure of the tumor upon it.

In all, the prominent symptom was excessive menstruation or hemorrhage.

All the patients were between the ages of 32 and 42. In all, the operation was successfully done without losing more than an ounce of blood ; without incising or tearing the cervix uteri or the vagina ; without inverting any portion of the organ by the continued traction ; without the subsequent establishment of pelvic peritonitis or cellulitis ; and without the loss of life as the result of the operation.

In all, the drainage tube, in its more or less modified form, was used.

The necessity of providing for thorough drainage and intra-uterine injections has been forcibly impressed upon me by the following clinical facts which have come to my observation :

That, in the natural condition of the organ, when there is an obstruction to the canal caused by a flexion, the intensity of the muscular contractions might be able to overcome the bend sufficiently to expel the blood clot, retained menstrual flow, or, at times, excessive uterine secretions ; but when there is a decomposing fluid or pus in its cavity, there seems to be present a state of atony whereby the contractile power of the uterus is lost, causing utter inability on its part to get rid of the purulent material.

That, in a large proportion of the cases of sub-mucous fibroids, the patients are greatly debilitated by prolonged hemorrhages, and wanting in both physical and nervous

tone. There exists a laxity of muscular fibre, partly due to diminished nutrition and partly to a diminished nervous stimulus, the result of which is, both a want of firmness in the structure of the uterus allowing a flexion and obstruction to the canal after the tumor is removed, and also a more ready absorption from its cavity on account of the greater looseness of its tissues.

That, whereas the contractile power of the uterus is undoubtedly greatly increased by the traction and the manipulation necessary in the removal of the tumor, yet after the operation, owing to debility, there is oftentimes present a state of utter inaction on the part of the uterus, rendering us powerless, despite ergot or stimulants, to arouse sufficient contractile power to expel a retained fluid from its cavity.

These cases are very different from those of ordinary labor, where the muscular fibre is developed, and where the nervous stimulus, if wanting, may be supplied by stimulants or electricity.

In the case of an ordinary fibroid, the force to be aroused by the stimulus is largely wanting.

These remarks would likewise apply to the removal of other growths than fibroids from the uterine cavity, as in Case 3, or in the removal of a large mass of cystic degeneration of the chorion, or any growth where there is to be any breaking down or disintegration of tissue causing a subsequent foul discharge.

The time to provide for this drainage is when the operation is done. The canal is then well open and the tube readily adjusted; and the delay required for its introduction does not amount to more than one or two moments. Indeed, there is no good reason why drainage should not be instituted in every instance of the removal of a submucous fibroid wholly within the cavity of the uterus. The

tube does not prevent perfect involution; for in each of the cases here reported, except Case 2, where death occurred from embolism, the uterus returned perfectly to its normal size, a fact verified both by bi-manual examination and by the passage of the uterine probe. Nor does its presence induce subsequent hemorrhages, there being no such history in any case where it was used. Let the surgeon who prefers to wait for symptoms of septicæmia before he provides suitable drainage or means of irrigation but once be forced to contend with the difficulties that are almost sure to arise—difficulties in the adjustment of the tube on account of the weakness of the patient, her sensitiveness to the touch, or her highly nervous condition; difficulties caused by constant blocking up with shreds of tissue on account of the small-sized canula which, at that late hour, he is obliged to use; or, if not actual difficulties, many slight vexations—I say, let him have but one such case to treat, and I am sure he will agree with me in the care of all subsequent ones.

My ideas regarding the removal of sub-mucous fibroids wholly within the cavity of the uterus, have undergone the most decided change in the last six years, in that then my advice was either to dilate repeatedly with tents, or at once incise the cervix and the capsule and give ergot, delaying all attempts at removal until the tumor presented at the os externum. In this treatment, I should have followed the teachings of Sims or Emmet; but I should also, I am sure, have subjected the patient, in thus doing, to months, perhaps years, of unnecessary suffering and possible hemorrhage; and also to needless dangers from the repeated use of tents, or from cutting the cervix or the tumor itself. Either of these processes I look upon as more serious than the actual removal of the tumor, the dilatation of the cervix preparatory to enucleation always being, to my mind, the most dangerous part of the whole work.

Now, in any case of suspected fibroid within the uterine cavity, I should advise immediate dilatation. This should be done in the inter-menstrual period, and as thoroughly as possible, using first a sponge tent, which seems best to soften the tissues; and substituting for it, after 8, 10, or 12 hours, as many laminaria or tupelo-wood tents as can be crowded through the os internum. The last-named tents do not expose the uterus to the prolonged septic influence that the sponge is apt to, if used throughout the whole process of dilatation. In as many more hours I should etherize the patient and explore the cavity with the forefinger. If the presence of a tumor be thus verified, remove it at once, even though it be small and project but slightly into the cavity, as in Case 4; for the danger of inflammatory action is greater in leaving it after what you have already done, than in pushing on and taking away the cause of the difficulty. If dilatation by the tents sufficient to allow the passage of the forefinger be not secured, I believe there is less risk in rapid dilatation with the hard-rubber plugs referred to, than in delay and the subsequent repetition of tents.

The practitioner is very apt to compare the process of dilating the non-pregnant uterus to that where pregnancy exists; a process with which he is generally more familiar, but which is totally different from it. In the one case he has to deal with a rigidity and resistance, particularly if the patient has never borne children, requiring a tremendous force to soften and overcome, and this force to be continued often for a long time. In the other he has the uterus softened by the influence of pregnancy, whose tissues yield readily to the application of a comparatively mild force for a short time.

Many authors advise the *écraseur* for this operation; but in my experience it has proved a cumbersome, annoying,

and worthless instrument, and I have twice been called to remove the remnant of a fibroid which the previous surgeon, using this instrument, had left behind. There is also greater danger, than with scissors, of cutting off a portion of a partially inverted uterus. As to the danger of hemorrhage and the advantages of the *écraseur* in controlling it, I do not believe, if Emmet's method of traction be practised, that such danger exists. I certainly have never seen any trouble from hemorrhage, either in my own practice or that of others, where this method could be used and the growth cut away with the scissors.

I would by no means have it understood that I advise drainage in every case of the removal of a fibroid. It must of course be limited to intra-uterine tumors, and is obviously non-essential in some of these; as, for instance, a fibroid polypus whose attachment to the uterus, even though it be to some portion of the interior of the body, is so small that the resulting denuded surface will be insufficient to give rise to any serious complications. Nor is it generally necessary when the fibroid has fully dilated the os internum, as the contraction of the cervix is not usually established until most of the disintegration has taken place. In the exceptional instance in the case of Dr. Sims, however, cited at the beginning of this paper, where the amount of tissue or shreds necessarily left is considerable, the practice is to provide drainage.

And I would, in conclusion, again strongly urge the use of the drainage tube after the removal of sub-mucous fibroids wholly within the uterine cavity, to be adjusted at the time of the operation, sewed to the cervix, and left in place till all discharge from the uterus has ceased.

## NEUROSES OF SENSATION OF THE PHARYNX AND LARYNX, OR SENSORY NEUROSES OF THE THROAT.\*

BY LOUIS ELSBERG, M.D.,  
NEW YORK,

PROFESSOR OF LARYNGOLOGY AND DISEASES OF THE THROAT IN DARTMOUTH MEDICAL COLLEGE.

### SECTION I.—DEFINITION.

**N**EUROSIS of sensation of the throat<sup>1</sup> is defined to be functional—and not merely subordinately symptomatic—disordered sensibility of the throat. (I mean to use the word *functional* neither “unphysiologically” nor “irrationally”: it does not imply with me that there is no physical basis for the particular derangement of function, but that the structural change is either remote from the organ the function of which is deranged, or that it is molecular and not discernible with the means of observation at our command.) The definition is meant to exclude, on the one hand, cases of neuritis and observed alteration in nerve tissue of the throat; and, on the other, throat neuroses which have no other than a symptomatic significance, as the increased sensibility and painfulness in inflammatory

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\* By special appointment prepared for, and partially read before, the International Congress in London.

<sup>1</sup>The term “throat” is here used technically, to denote all the parts which at the present day the throat-specialist, or laryngologist, is called upon to treat: *par excellence*, therefore, the larynx and pharynx.

affections, the diminished or abolished sensibility during narcosis and hypnotism, in anaesthetic lepra, and in connection with the failure of all the powers, such as during an epileptic fit, in cholera, and in approaching death from any cause, and the perverted sensibility occasioned by the actual presence of foreign bodies, tumors, etc., as well as the misinterpretations of sensibility which result from delusive conceptions in primary insanity.

#### SECTION II.—ANATOMICO-PHYSIOLOGICAL BASIS.

Many questions relating to the normal and pathological sensibility of the larynx, pharynx, and the other constituents of the throat are not yet definitely settled.

It is well known that normal sensation involves the integrity of three factors, viz., of the nerve ends which receive an impression, of the conducting substance which conveys the impression, and of the central organ which receives the impression afterward transformed into conscious sensation. My definition of sensory neuroses of the throat excludes the consideration of structural throat lesions, involving the peripheral sensitive nerves, and of the brain lesions of insanity. In all other cases, there must be affected either the extra-axial or intra-axial nerve tracts or the nuclei immediately connected with them. Investigators agree that the nerves with which we are concerned are mainly those that go to make up the pneumogastric, and, as to the fauces and pharynx, to a large extent, also, the glosso-pharyngeus. As is well known the larynx obtains its nerves of sensation almost exclusively from the superior laryngeal nerve which emanates from the plexus gangliformis of the pneumogastric,—a plexus in which enter, besides the vagus, fibres from the accessory and hypoglossal nerves, and which receives anastomotic branches from the glosso-pharyngeus, the first cervical ganglion of the sym-

pathetic, and the cervical plexus. There are a few sensory filaments contained in the inferior laryngeal nerve, which are given off with the nervus arytaenoideus,—the terminal of one, namely, the external, of two branches (sometimes sets or bundles) into which the inferior laryngeal nerve divides. The immediate source of the sensory nerve supply of the pharynx is the pharyngeal plexus which is composed of the pharyngeal branches of the pneumogastric with which are mixed the pharyngeal branch of the glossopharyngeus and the sympathetic. The pneumogastric in connection with the glossopharyngeus supplies the inner sides of the palatine folds, the fauces and tonsils as well as the posterior side of the velum palati. The second branch of the fifth supplies the anterior face of the velum and the region of the pharyngeal orifice of the Eustachian tube and vault of the pharynx.

While, thus, the derivation of the sensitive nerves of the throat from these larger nerves is clear, of their connection through the nerve roots with their immediate centres, nothing positive is known. It has, however, been suggested to me by Dr. E. C. Spitzka, of New York, in a personal communication, that by a process of anatomical exclusion we may infer where their nuclear centre probably is. His reasoning is as follows: Taking the ninth, tenth, and eleventh pairs of cranial nerves, *i. e.*, the glossopharyngeal, pneumogastric, and spinal accessory, in the aggregate, it is found that they have in common three nuclei. These are, 1. a *sub-ependymal* nucleus, which experiment as well as several anatomical facts justify us in regarding as a nucleus for visceral innervation. 2. A large multipolar-celled nucleus, devoted presumably to the innervation of the laryngeal muscles. (Certain authors incline to the belief that this is really an origin of the hypoglossal nerve<sup>1</sup>; and

<sup>1</sup> Laura, Mem. della reale accademie delle scienze di Torino, serie II, j. 31 and 32.

Krause<sup>1</sup> considers its relations so much in doubt that he terms it *nucleus ambiguus*, but there are no good reasons for refusing assent to the proposition of Meynert<sup>2</sup> that it contributes its efferent fibres to the oblongata-portion of the eleventh pair, in other words, to that part of these nerves which through its subsequent fusion with the tenth pair innervates the muscles of phonation.) 3. The *nucleus pharyngeus*,<sup>3</sup> a mass of cells situated near the lateral field of the oblongata, which from its multipolar cells and the fact that it is found in best development in the levels of the ninth and tenth pairs is supposed to be devoted to the innervation of the pharyngeal muscles.

There is a fourth nucleus, viz., the gelatinous substance with small nerve cells scattered around the ascending root of the fifth pair. Now, as the sub-ependymal nucleus is probably visceral and the deep nuclei in all likelihood motor, the sensory innervations naturally fall to this nucleus, which is documented as a tactile and trophic centre by its relations to the great cranial tactile nerve, the fifth.

A fact which certainly strongly points to this conclusion is that while the spinal accessory nerve roots never pass through or receive accession from this fourth nucleus, the ninth and ten pairs do pass through and do receive such accession.<sup>4</sup> Here then we may look for the tactile innervations of the larynx and pharynx, and the field from which certain reflexes are mediated to the motor nuclei. The analogy with the spinal reflex structure would thus be complete; for the gelatinous head of the posterior horn of the cord being homologous with the gelatinous nerve nucleus

<sup>1</sup> Krause, Allgem. und mikroskop. Anatomie. Hanover, 1876.

<sup>2</sup> Meynert, Vom Gehirne der Säugethiere. Stricker's Handbuch.

<sup>3</sup> Spitzka, "Architecture of Brain," *Journal of Nervous and Mental Diseases*. 1880.

<sup>4</sup> Meynert, *op. cit.*; Stilling, "Ueber die Textur der Medulla oblongata," Erlangen, 1842.; Spitzka, *loc. cit.*

of the trigeminal root, the nuclei of the pharyngeal and laryngeal muscle-nerves being analogous to the anterior cornu and their efferent fibres analogous to the anterior root, the afferent fibres of the ninth and tenth pairs represent so many posterior root-fibres and complete the reflex arch. No opportunity for an autopsy where this nucleus might have been affected has occurred to me since Dr. Spitzka's suggestion; but in a case, which has turned out to be progressive paralytic dementia, in which for some time disease seemed to be confined mainly to this domain, we may possibly in the future have light thrown on this point by *post-mortem* examination.

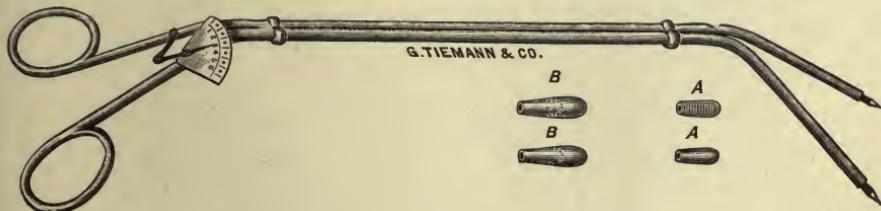
The normal sensibility of the throat varies in different portions and in different persons, even at different times in the same person; nevertheless, there is some agreement within recognizable limits. The exceptional cases of hypersensitiveness and of want of sensitiveness met with, within the latitude of health, in performing laryngoscopy and various surgical operations, are not sufficient to disturb the rule. Excessive variations must be regarded as being abnormal.

The only published investigations as to the sensitiveness of the laryngeal mucous membrane are those of Pienaczek.<sup>1</sup> He found that temperature was distinctly appreciated; that while tactile perceptions were usually covered by the irritation which the touch of bodies caused, yet when habit had blunted the irritability and the reflexes were restrained by the parts having become accustomed to the touch, differential tactile appreciation seemed to take place; and that, the perception of pain was much less in the larynx than in the skin. After repeated similar examinations by means of various probes, blunt-pointed and sharp, and all sorts of throat instruments, metallic and of different materials, I

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<sup>1</sup> Über die Empfindlichkeit der Larynxschleimhaut." *Medizinische Jahrbücher*, Vienna, 1878 p. 481.

can express myself in entire accord with Pienaczek, as far as he goes. I found the appreciation of temperature acute in every portion of the throat; next in distinctness of sensation were differences in pressure, and thereby, to a slight extent, consistence and form. Without going deeply into physiological disquisition, I must recall the interesting observations on acuteness of sensibility of different portions of the body made nearly half a century ago by E. H. Weber.<sup>1</sup> He measured the degree of sensibility by the distance between the two legs of a pair of compasses which different parts require in order that the two points make distinct impressions. Aside from the skin he examined the mouth, and found that at the tip of the tongue the two points were separately felt when only half a line apart, while on the dorsum and on the edge of the tongue (one inch from the tip) they had to be four lines apart, and that the mucous membrane of the hard palate required the distance to be six lines, and over the gums nine lines, before the sensation of two points and not of one was produced. Weber assumed that the difference of sensibility depends upon the number, course, and termination of the nerve filaments in the different portions. On account of the irritation, which touching the throat with any instrument usually produces, it is very difficult to arrive here at any accurate æsthesiometrical results. Somewhat after the model of Ziemssen's double laryngeal electrode, I have devised a con-



<sup>1</sup> *Annotat. Anat. et Physiol.*, pp. 44-81. Quoted by Dr. Johannes Müller, "Handbuch der Physiologie des Menschen." Coblenz, 1837, vol. i, p. 711.

venient throat æsthesiometer, which, as shown in the accompanying wood-cut, essentially consists of two rods properly bent, so connected together that their points can be easily approximated and separated, the distance being measured on a scale at the handle. A little way from the extremities, screw-threads commence for the purpose of carrying tips, some of which are metallic, marked *A* in the figure, while others are made of very soft rubber, marked *B*.

From investigations in a number of healthy persons I can state that the normal distances between two uncovered points of the æsthesiometer necessary for their distinct appreciation vary from  $1\frac{1}{2}$  or 2 to  $2\frac{1}{2}$  or 4 centimeters. This, of course, precludes the possibility of measuring by such distances the sensibility of very circumscribed spots. Besides, practice changes the distance, and the examination is by no means easy. More important than the examination of the distances is that of temperature and pressure. Normally very slight differences of temperature are appreciated. I examine these with my æsthesiometer, the two points mounted with metallic tips, each dipped into water of a different temperature (or otherwise heated and cooled), thus constituting a differential thermæsthesiometer. To measure appreciation of consistence, etc., *i. e.*, pressure, I use the soft-rubber tips. The application of electricity furnishes the best means of measuring the sensitiveness to pain. I discriminate between three different kinds of normal as well as abnormal sensibility of the throat, viz.: 1. *tactile*, by which temperature and pressure are appreciated; 2. *dolorous*, by which pain is appreciated; and, 3. *reflex*, from which result muscular contractions, such as cough, spasm, gagging, choking, etc., as well as intravascular and secretory phenomena. Each of these kinds of sensibility must be examined æsthesiometrically; the first, as I have above described; the second, by an elec-

trode, connected first with an induction machine and then with a constant battery, and also by a sharp-pointed probe, or my æsthesiometer uncovered; and the third by a blunt-pointed probe, or my æsthesiometer covered. Under the head of diagnosis I shall speak again of this examination, and especially of the examination of dolorous and reflex sensibility.

As to what I call dolorous sensibility, there has been some discussion whether or not the sensation of pain ought to be considered as a function of sensitive nerves. Anstie<sup>1</sup> insists that it ought not; but in spite of his verbal distinction that "it is not the *function* of sensitive nerves to convey the sensation of pain but only their *action* under the presence of extraordinary influences," I think it may be stated without fear of serious contradiction, that to mediate the appreciation of a certain amount of pain in response to appropriate impressions is their normal function, and that any alteration of this mediation, whether a diminution or an increase, beyond the limits of individual healthy variation, as well as a spontaneous sense of pain, constitutes a disorder of sensibility, *i. e.*, a dysæsthesia.

Krishaber<sup>2</sup> was the first who drew attention to the fact that the sensibility of the larynx is of two kinds, viz.: a common or general sensibility (which includes the two kinds I call tactile and dolorous) and a reflex or special sensibility, and that a sensory disorder may affect one or the other of these, or both; but I do not agree with him as to his definition of laryngeal reflex sensibility. He pointed out that if we cauterize the mucous membrane of the larynx, we provoke a number of noisy and painful inspiratory movements, convulsively closing the larynx, while expiration remains calm and deep. There is pain for some hours or minutes

<sup>1</sup> "Pharyngeal and Laryngeal Neuralgia." *Neuralgia and the Diseases that resemble it.* New York, ed. 1872, p. 113.

<sup>2</sup> *Dictionnaire Encyclop. des Sciences Méd.*, Dechambre. Paris, 1872, 2me série, vol. i, p. 677.

according to the strength of the caustic, but *no cough*. If, instead of cauterizing, we carry a simple drop of water to the part, thus imitating what frequently happens in so-called "wrong swallowing"—swallowing the wrong way,—we also provoke convulsive movements of the laryngeal muscles, but the phenomena are entirely different from those in the former case. The most prominent phenomenon now is a violent cough, a cough hard and loud, for the production of which all the expiratory muscles are called into action; and while inspiration, though in the beginning of the paroxysm perhaps much embarrassed, soon becomes calm and normal, the cough continues as long as there is the least disagreeable sensation in the larynx. Only to the sensibility excited in the latter case Krishaber accorded the name reflex sensibility; that in the case of cauterization he regarded as the general or common sensibility; the first being followed, as he said, by tickling and cough, the latter by pain and local spasm; the first a special sensibility relating to an occasional function, the expulsion of matters from the air passages, being connected with expiration; the latter a general sensibility relating to a permanent function, the permeability of the *rima glottidis* (the perturbation of which can give rise to a formidable accident, closure of the air passages by spasm), being connected with inspiration. Now, although the discrimination which Krishaber made between expiratory and inspiratory convulsive movements is an important one, both phenomena are reflex. Not only both cough and spasm, but, as I have already stated, other muscular contractions and circulatory and secretory actions may result from excitation of the reflex sensibility of the throat.

Taking the three kinds of sensibility together, I have found in two persons out of three—and at my request Dr. Ephraim Cutter examined his own very tolerant throat and arrived at substantially similar results—that the angles of

the posterior wall of the pharynx just behind the posterior palatine folds, and the posterior wall of the larynx, are the most sensitive portions of the throat. Next comes the laryngeal face of the epiglottis, while the edge of the velum is the least sensitive. The different portions vary in the following order, viz.: 1. Angles of pharynx above described. 2. Posterior laryngeal wall and inter-arytenoid fold. 3. Laryngeal face of epiglottis. 4. Valleculæ and root of tongue. 5. Lower laryngeal cavity (when reachable). 6. Trachea (when reachable). 7. Lateral laryngeal walls. 8. Ventricle of Morgagni. 9. Arytenoid cartilages. 10. Pyriform sinuses. 11. Palatine folds. 12. Tonsil. 13. Glosal face of epiglottis. 14. Free edge of epiglottis. 15. Ary-epiglottic fold. 16. Fauces. 17 Lateral walls, and low down on the posterior wall, of pharynx. 18. Vocal bands. 19. Uvula. 20. Velum, on the sides. 21. Velum, in the centre and on the edges.

(*To be continued.*)

## TRANSIENT ALBUMINURIA, AS IT OCCURS, PARTICULARLY IN CHILDREN AND ADO- LESCENTS, IN APPARENT HEALTH.\*

By FRANCIS P. KINNICUTT, M.D.,

PHYSICIAN TO ST. LUKE'S HOSPITAL AND TO THE OUT-PATIENT DEPARTMENT, NEW YORK HOS-  
PITAL, ETC.

THE occurrence of a transient albuminuria in persons in apparent health, and particularly in children and adolescents, is a subject which has attracted the attention of numerous clinical observers and pathologists during the past few years, and is certainly one of much practical importance. Cases have been reported by the English physicians Sir William Gull, Drs. George Johnson, Moxon, Saundby, Dukes, and T. Morley Rooke, and by Leube, Fuerbringer, Edlefsen, and Ultzmann among other German observers.

Before further reference to and analysis of their observations and a number of my own, I shall briefly review the various theories which have been advanced in explanation of the conditions which govern this form of albuminuria. Accepting Nussbaum's<sup>1</sup> conclusions, based upon experimental research, that the site of transudation of albumen in the

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\* Read before the New York Academy of Medicine, Dec. 15, 1881.

<sup>1</sup> Nussbaum, M.: Fortgesetzte Untersuchungen über die Secretion der Niere; *Pflüger's Arch. f. d. gesammte Physiologie der Menschen und der Thiere*, 1878, xvii, 581. Ueber die Entstehung der Albuminurie; *Deutsche Arch. f. Klin. Med.*, 1879, xxiv, 248.

kidney is in the glomerular vessels, Runeberg<sup>1</sup> maintains, as the result of numerous experiments by himself, that the filtration of albumen is dependent upon an increased permeability of the walls of these vessels, due to a condition of diminished or low blood pressure. The filtration membranes used by Runeberg in his experiments were dogs' sheeps' and rabbits' intestines, suitably prepared. Heidenhain asserts that Runeberg's conclusions are untenable in view of the demonstrable faultiness of his experiments. He shows by an analysis of his results, "that while the percentage of albumen in the filtrate diminishes by increased pressure, the absolute quantity of albumen is actually increased. With increased pressure both more albumen and more water are filtered, but the albumen stream increases more slowly than the water stream, so that the percentage of albumen in the filtrate relatively decreases with heightened pressure."<sup>2</sup>

He also shows that the membranes used by Runeberg differ in their filtration properties from the glomerular membranes, and that in other respects there is an essential difference between "the physical experiment and the physiological process."

Wittich has advanced the theory that the urine which passes through the glomerular capillaries constantly contains albumen, but that this albumen serves the purpose of nourishing the epithelium of the urinary tubules, and that the residue passes back into the circulation. In a diseased condition of the epithelium, or in its removal from any cause, albuminuria would occur. An objection which has been urged against this hypothesis, is that it assumes a different mode of nourishment of the renal epithelium from

<sup>1</sup> Runeberg, J. W.: Die Filtration von Eiweisslösungen durch thierische Membranen; *Arch. f. Heilk.*, 1877, xviii, I. Albuminurie bei gesunden Nieren; *Arch. f. Path. Anat.*, 1880, lxxx, 175.

<sup>2</sup> Heidenhain, R.: Hermann's "Handbuch der Physiologie," 1880, Bd. v, 367.

all other epithelium, while it fails to satisfactorily explain the transient albuminuria following an epileptic attack, occasionally cold bathing, and occurring in the cases recorded in the present paper. Rosner, moreover, has adduced an experimental objection to this theory. By placing fresh bits of kidney in boiling water, he was able to fix the albumen in the situation in which it was produced. In diseased kidneys coagulated albumen was found in the urinary tubules, but never in healthy organs. Another explanation, which has been suggested, is that the epithelial covering of the smallest capillaries prevents the transudation of albumen; denuded of their epithelium, albumen escapes.

The experiments of Stokvis and others would seem<sup>1</sup> to demonstrate the incorrectness of the theory that the filtration of albumen is dependent solely upon a condition of increased or high pressure in the glomerular vessels. Stokvis has shown that when the pressure is increased in the renal arteries either by ligating the aorta below their origin, or through the extirpation of one kidney, albuminuria does not occur until the degree of pressure is such as to rupture the vessels and permit of the admixture of blood with the urine. The clinical observations that in the case of the small kidney, which is so constantly associated with high arterial tension, albuminuria as a rule occurs only occasionally and to a limited degree<sup>2</sup>; and, again, that in those forms of heart disease attended with obstructed venous circulation, albumen does not appear in the urine until the general arterial tension, and, consequently, that in the glomerular vessels, is lowered, through failure in the heart's action, while remedies which directly

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<sup>1</sup> "Cyclopaedia of the Practice of Medicine," Ziemssen, Supplement, 1881, p. 661.

<sup>2</sup> Mahomed's explanation of the diminished filtration of albumen in this condition, on the ground of a maintenance of balance between thickness of vessel walls and arterial pressure (*Lancet*, vol. i, p. 76, 1879), I cannot accept without reservation, in view of some personal observations, which I hope, at another time, to report.

tend to increase its force diminish or arrest the albuminuria, are in accord with the physiological experiment.

Other theories to which the transudation of albumen have been referred are a retardation of the blood current in the renal vessels<sup>1</sup> (Heidenhain and others), and Leube's hypothesis<sup>2</sup> of an individual permeability of membrane. It was shown in the experiments of Heidenhain and Runeberg (*l. c.*) that the filtration of albumen through animal membranes goes on very slowly; in other words, that it is an essential condition of such filtration, that the albumen should remain a comparatively long time in contact with the vessel wall. A vaso-motor disturbance within the kidney (either a paralysis of the vaso-constrictors or an irritation of the vaso-dilators), by producing a dilatation of the vessels, would fulfil this condition, viz., a retardation of the blood current. That this condition should alter the nutrition of the glomerular epithelium, and, when temporary, should produce a transient disturbance of the functions of the latter, is readily conceivable. It has seemed to me that the above theory, viz., a slowing of the blood current in the glomerular vessels, dependent upon a temporary vaso-motor disturbance, with a resulting alteration (also temporary) in the functions of the glomerular epithelium, may be regarded as the most probable explanation of the mode of production of transient albuminuria in persons presumably healthy. It remains for us to seek for the cause of such a vaso-motor disturbance. Inasmuch as my observations have been confined chiefly to children and adolescents, in whom, from the time of life, apparent health, and disappearance of the albuminuria (in the cases recorded), structural disease of the kidney may reasonably be excluded, I shall not venture to extend my conclusions to a later period in life.

<sup>1</sup> *Loc. cit.*

<sup>2</sup> Leube, W.: Ueber die Ausscheidung von Eiweiss im Harn des gesunden Menschen; *Virchow's Archiv*, 1878, lxxii, 145.

From a careful analysis of cases recorded by other observers and a study of my own, I have been led to believe that the source of irritation, in a large number of instances, is to be found in the temporary presence of imperfectly oxidized nitrogenous matters in the renal circulation; in other words, to a transient oxaluria or lithuria, which, clinically, may be regarded as identical. The marked derangements of the nervous system in more or less chronic lithæmia in adults are well recognized; many of the symptoms which Dr. Da Costa<sup>1</sup> has recently made the subject of a very interesting monograph indicate the degree to which the vaso-motor system is affected. In children, the general nervous symptoms attending the lithæmic state are equally pronounced, though hitherto not as well recognized. To this fact my attention was first drawn by Dr. Wm. H. Draper, and my experience since has fully corroborated it. That the nervous symptoms are directly dependent upon the presence in the blood of the irritating products of imperfect oxidation of nitrogenous matter can hardly be doubted, although different views may be held as to the system which is primarily concerned in the production of this state.

With these facts before us we may reasonably believe that a more or less transient oxaluria or lithuria may be capable of producing a correspondingly temporary irritation of the nervous system. A transient general nervous disturbance, in which the vaso-motor system would necessarily share, may be supposed, or one confined to the vaso-motor system of the kidney in its excretion of a large amount of irritating matter, and resulting in a temporary albuminuria.

A similar albuminuria which follows an epileptic attack, may presumably be referred to the disturbance which affects the general nervous system, and consequently, the vaso-motor system of the kidney.

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<sup>1</sup> Da Costa, *American Journal of the Med. Sciences*, Oct., 1881.

Again, in the temporary albuminuria which has been observed in cases of exophthalmic goitre,<sup>1</sup> we have a probable instance of retarded blood current in the renal vessels, the result of dilatation dependent upon the affection of the general vaso-motor system.

The apparently more frequent occurrence of temporary albuminuria, without structural change in the kidney, in children and adolescents, than at a later period, I would explain on the ground of the greater mobility of the nervous system which obtains at these periods of life.

Its occurrence in only a comparatively small proportion of cases of lithæmia at these ages may be referred to an *individual* mobility of the nervous system in such cases. (In another group, the skin or the mucous membranes may be the most vulnerable portions of the organism.) In my own experience, a transient albuminuria in connection with the lithæmic state has been observed, as a rule, in children and youths of nervous temperament; in the latter instance, in active brain-workers, frequently during periods of unusual mental worry or strain; under conditions where the general nervous tone was below par. (In this connection a simple reference to the clinical fact of the influence of profound mental emotion in temporarily increasing the albuminuria in chronic Bright's disease is suggestive.) Leube's hypothesis of an individual permeability of membrane (*I. c.*) may also be considered as a possible etiological factor in these cases.

In illustration of the subject which we have been considering, I shall now briefly review the cases of transient albuminuria which have been recorded by various observers.

In a discussion before the Royal Medical and Chirurgical Society of London in 1873, on albuminuria,<sup>2</sup> Sir William Gull mentioned that in his experience "it occurred in

<sup>1</sup> Dr. Begbie, *Edinburgh Med. Journal*, April, 1874.

<sup>2</sup> *Lancet*, 1873, i, 808.

young and growing men and boys, almost as frequently as spermatorrhœa," and that it might presumably be referred to an atony of vessels and nerves. In a paper by Dr. Moxon ("Guy's Hospital Reports," 1878) 19 cases of intermittent albuminuria occurring in adolescents in apparent health are reported. The symptoms presented by the patients were of an indefinite character, consisting in listlessness, languor, occasional headache, unrefreshing sleep, with little disposition for cheerful company. In the majority of cases no complaint was made by the patients, but advice was sought through the anxiety of friends. Dr. Moxon says: "I have not met with this set of conditions at other periods of life, and I have so frequently met with them in adolescents, that I cannot but believe that it is a disordered state proper to that term of life, and that it is deserving of recognition and receiving a name." He further mentions that careful examination, in his cases, of the various organs of the body gave negative results, and that, moreover, he was able to exclude scarlatina and diphtheria, which might leave a sequel of albuminuria and lead to fallacy. Many of the cases were kept under observation for several years, the urine being repeatedly examined, with the result of finding that the albuminuria had wholly disappeared, and that excellent health was maintained.

In the history of the various cases, whenever the result of the examination of the urine, aside from the existence of albumen, is recorded, the presence of a large amount of oxalate of lime, or a combination of uric acid and oxalate of lime, is mentioned. Their presence, and in large quantity, was so commonly observed as to attract Dr. Moxon's attention. He does not, however, express any positive opinion as to their causal relation to the albuminuria. It may be mentioned that small oxalate of lime calculi were subsequently passed by one of his patients.

In the *British Medical Journal* of Nov. 10, 1878, ten cases of intermittent albuminuria in adolescents from 13 to 17 years of age are reported by Dr. Clement Dukes. In three of the cases there is presumptive evidence of the existence of structural change in the kidney. In Case 1 the albuminuria followed an acute attack of nephritis with bloody urine, and occurred at intervals for the subsequent eight months, when the record ceases.

In Case 2 albumen was detected in the urine immediately subsequent to an attack of scarlatina, and it continued to appear from time to time up to the date of publication of Dr. Dukes' article.

Finally, in Case 10 it is stated that with pallor and a feeling of faintness on severe exertion, high arterial tension existed, and an examination of the heart revealed a heaving, hard impulse—suspicious symptoms certainly in connection with albuminuria. Inasmuch as in only three of the remaining cases had the albuminuria disappeared at the date of record, and in these for a comparatively short time, we are hardly warranted in positively assigning them to the group which we have under consideration. They certainly differ in many respects from the cases described by Dr. Moxon, and from my own. The symptoms consisted of furred tongue, headache, disinclination for work or play, irregular or constipated bowels; often there was anaemia, and occasionally a slight cold, facial dropsy, and slight syncopal attacks. The microscopic examination of the urine is not given. In Dr. Dukes' opinion the albuminuria is the result of a temporary hyperæmia of the kidneys, super-added to an habitual increased arterial tension, which he claims obtains at puberty, basing this opinion upon Beneke's researches. Mahomed, on the contrary, asserts that the result of his personal experience is to the effect that a condition of low tension exists at this period of life.<sup>1</sup>

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<sup>1</sup> *Lancet*, 1879, vol. i, p. 76.

In an article in the *British Medical Journal* of Nov. 12, 1881, Dr. Dukes reverses his former opinion, stating that as the result of further observation he is forced to regard the albuminuria of adolescents not only as pathological, but as representing the beginning of true Bright's disease. He adds, however, that he has been unable to observe his cases in its further development.

In a single case of transient albuminuria recorded by Dr. Yeo (*British Med. Journal*, Oct. 26, 1878), the presence of albumen in the urine alone being mentioned, an explanation of the symptom is sought in the hypothesis of a temporary vascular asthenia, dependent upon a possible deficiency or disturbance of nerve force of transient duration.

In an intermittent albuminuria in a young girl, disappearing during rest in bed,<sup>1</sup> Dr. T. Morley Rooke seeks to explain the condition on mechanical grounds. He says that "when the body is in the upright position the weight of the column of blood is too great for the weakened vessels,"—a theory which requires further demonstration.

In a very interesting paper in the *British Med. Journal* of Dec. 13, 1879, on "Latent Albuminuria, its Etiology and Pathology," Dr. George Johnson mentions the frequency with which albumen is found in the urine of persons in apparent health, and terms it "latent albuminuria." He maintains that although unassociated with any evidence of functional or structural disease, it may, by careful inquiry, be traced back, in a very large proportion of cases, to some possible exciting cause, *i.e.*, to an attack of scarlet fever or diphtheria, or a cold, etc.; moreover, "that even the smallest trace of albumen in the urine is always pathological, never physiological; and that the neglect of a pathological condition and tendency, especially such negligence

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<sup>1</sup> *British Med. Journal*, Oct. 19, 1878.

as involves repeated exposure to the exciting cause, may convert a temporary and occasional into a persistent albuminuria, which sooner or later, though it may be after many years, will result in a fatal disintegration of the kidneys." Admitting the frequent dependence of albuminuria upon dyspepsia, he believes that the chain of events is represented by an irritation of the gland cells of the kidney in the process of excretion of ill-digested matter, through which they later undergo structural changes, while at the same time the imperfectly assimilated albuminous materials pass more readily by exosmosis through the Malpighian bodies. An additional factor is found in the general nervous exhaustion, with loss of vaso-motor force, which accompanies chronic dyspepsia, leading to a diminution of tone and contractile power in the muscular walls of the arterioles generally, including those of the kidney. Admitting, therefore, the irritating effect of the products of mal-assimilation upon the kidney, as well as a vaso-motor disturbance accompanying their presence in the blood, he believes apparently that the gland cells are primarily affected, undergoing structural change with a continuance of the irritation, rather than that such irritation may be confined to the vaso-motor system within the kidney, with resulting temporary disturbance in the nutrition and functions of the glomerular epithelium, and consequent transient albuminuria.

In an examination of the urine of 145 male patients by Dr. Saundby, taken *seriatim* as they presented themselves at the General Hospital in Birmingham, England, albumen was discovered in no less than 105<sup>1</sup>; 67 of these cases are tabulated by Dr. Saundby under the head of chronic Bright's disease; five cases, occurring in patients between

<sup>1</sup> Saundby, R.: The Diagnostic Value of Albuminuria; *Brit. Med. Journ.*, 1879, i, 699.

ten and twenty years of age, are attributed to simple debility; two, at a similar period of life, to dyspepsia.<sup>1</sup> Of these a portion, he says, resembled Dr. Dukes' cases, a portion Dr. Moxon's.

No evidence of organic disease could be detected. The examination of the urine is not given in full, but it is stated that in a few instances oxalate of lime was present. In Dr. Saundby's opinion a temporary hyperæmia may be regarded as the source of this form of albuminuria, and he believes that it is in no respect inconsistent with an integrity of renal structure.

In a case of intermittent albuminuria, in a physician, 29 years of age, in good health, which finally disappeared after an interval of eight months, reported by Fuerbringer,<sup>2</sup> there are several points of interest. The albumen was first accidentally discovered in a morning specimen of urine, passed after an hour of great mental anxiety. The noon urine of the same day contained less, and the evening water, after the mental depression had passed away, was entirely free from albumen. During the following week occasional traces of albumen were detected. After a second period of great anxiety and depression a small quantity of dark, clear, highly acid urine was passed, of a sp. gr. 1,030, containing 31 per cent. of albumen and a large amount of uric acid. Under similar circumstances, on two subsequent occasions the same symptoms occurred. At other times, when the amount of albumen was less, uric acid, or oxalate of lime, or both, with amorphous urates, were commonly present. No effect upon the albuminuria was produced by diet. The ingestion of eggs in large number, strong spices and liquors, were followed by negative results. Severe physical exercise produced a temporary in-

<sup>1</sup> In one of the cases classed under the head of debility, the albuminuria followed typhoid fever.

<sup>2</sup> *Zeitschrift für Klinische Medicin*, 1880, i, p. 340.

crease in its amount. A very careful examination of the patient failed to reveal any organic disease. Fuerbringer would explain the phenomena on Runeberg's theory of diminished pressure in the glomerular vessels, produced by general arterial ischæmia, the result of profound emotional disturbance of a depressing character. The influence, however, of similar causes in exciting a temporary lithæmia is well recognized, and I would therefore suggest that profound mental depression, lithæmia, albuminuria constituted the probable sequence of events in the above case.

The following cases have come under my personal notice. Within the past few years the number of such observations has been comparatively large, but as they have all been made in private practice, the opportunities for carefully tracing the greater number of cases have been wanting. I shall, therefore, with a single exception, confine my remarks to such as I have been permitted to keep under constant observation, and where the albuminuria has now been absent for many months. In all of them careful inquiries were made in regard to previous illnesses, which might have a bearing upon the subject under consideration, with results which will be stated in the individual cases. The possible existence of affections of the bladder and urethra were carefully investigated, but they were invariably found to be absent. In this connection Simon's researches, to the effect that neither the prostatic nor seminal fluids contain any bodies which are coagulable by heat, may be mentioned. "Casts" were never found in the urine, although careful search was made for them. In none of the cases was the amount of urinary water apparently decreased, although the quantity passed in the 24 hours was not measured.

Sphygmographic tracings, which were taken whenever possible, showed either normal or slightly lowered arterial tension. In all the cases the amount of albumen in the

urine was very considerable. In all, the microscopic examination was made within *twelve* hours; in many of the cases, as soon as *two* hours after the passage of the urine.

At the period when the following case was observed my attention had not been directed either to the occurrence of transient albuminuria in health or to its association with a temporary oxaluria or lithuria. I believed it, at the time, to be one of latent Bright's disease.

CASE 1.—Mr. A., aged 23. The patient is of nervous temperament. While in New York on a visit in the winter of 1874, in apparent health, his urine was examined by me in a non-professional capacity. To my surprise and concern it was found to contain a large amount of albumen, together with much uric acid and oxalate of lime. Without mentioning the matter to Mr. A., I immediately wrote to his family physician in regard to the result of my examination. I received in reply a communication to the effect that the urine had been examined by him on Mr. A.'s return home, two days later, and was found to be wholly free from albumen. His water has been very frequently examined since with a similar result, and his health has continued to be excellent.

CASE 2.—Mr. B., medical student, aged 21. Is of nervous temperament. Had scarlet fever without albuminuria during childhood. Has never had diphtheria. Has been under my observation for the past five years, during which period he has enjoyed excellent health. In February, 1881, while engaged in active brain work, taking little exercise or recreation, but eating largely, he began to complain of lassitude, weariness after exertion, either mental or physical, and slight morning headache. An examination failed to reveal any evidence of organic trouble. Examination of the urine gave the following results : React. highly acid ; sp.gr. 1,030; albumen, both by heat and nitric acid. Acidulated with a few drops of acetic acid, boiled, and allowed to stand, albumen  $\frac{1}{10}$ . Microscopic examination showed the presence of a large amount of uric acid and oxalate of lime, but was otherwise negative. Hunyadi Janos water on rising, with alkalies before meals, a regulation of the diet, and exercise in the open air were prescribed. The urine continued to contain albumen in diminished amount for the three following days, when the record of the exam-

ination is as follows : React. neutral ; sp. gr. 1,020; no trace of albumen obtained either by heat or nitric acid. Microscopic examination negative. There was accompanying improvement in the general symptoms. During the past ten months the urine has been repeatedly examined (last date Dec. 1, 1881), but never at any time has it contained albumen ; uric acid and oxalate have also been constantly absent.

CASE 3.—Mr. B., student, aged 17. Has never had diphtheria. Had scarlet fever five years ago, without albuminuria. The urine was repeatedly examined by me during and subsequent to his illness, and at no time contained a trace of albumen. Has always enjoyed excellent health. Is a large eater. In Oct., 1880, while in apparent perfect health, on account of a remark that there was a sediment in his water, the urine was examined. The record is as follows : React. highly acid ; sp. gr. 1030 ; albumen, both by heat and nitric acid. Acidulated, boiled, and allowed to stand, albumen  $\frac{1}{2}$ . Microscopic examination : A large amount of uric acid and oxalate of lime present ; otherwise negative. A saline cathartic, alkalies, regulation of diet, and out-door exercise were prescribed. An examination of the water 36 hours later showed a normal sp. gr. and acidity, an entire absence of albumen, of uric acid, and oxalate of lime. The urine has been repeatedly examined since (last date Nov. 25, 1881), but albumen has never been found ; neither has it contained uric acid or oxalate of lime. The patient's health has continued excellent.

In the above cases the marked degree of the albuminuria, its very transient nature, its constant association with uric acid and oxalate of lime are noteworthy.

CASE 4.—Mrs. D., aged 22. Patient is of nervous temperament and lithæmic diathesis. Has never had diphtheria. Had scarlet fever, without any evidence of renal trouble during childhood. Has been under my observation for several years, during which time she has enjoyed excellent health.

The usual routine examination of the water was made on several occasions during this period, in trifling ailments, but albumen was never found. Late in the past autumn my advice was sought for the relief of morning headache and general lassitude. The patient also complained of moderate constipation, flatulence after meals, and a coated tongue. There was no fever or other

evidence of more than a temporary functional disturbance of health. For several weeks previous she had been living a very luxurious life, eating heartily and taking little exercise.

An examination of the urine, an after-breakfast specimen, showed a highly acid reaction, with a sp. gr. 1030; very considerable albumen both by heat and nitric acid, and oxalate of lime in large amount. An evening specimen of the same date resembled the morning's in all respects, with the exception of possessing a higher sp. gr., viz., 1032. Advice, similar to that which has been mentioned in the previous cases, was given and adopted, with the result of speedily relieving the subjective symptoms. Examination of the urine four days later showed a diminution in the acidity, lower sp. gr., and a mere trace of albumen in the evening specimen only. Uric acid was also found in the evening water alone and in small quantity. Examination of the urine on the fifth subsequent day: react. acid; sp. gr. 1022; no trace of albumen on very careful testing with both heat and nitric acid, in either a morning or evening specimen. Microscopic examination, negative. Two weeks later the examination was repeated. The record is as follows: After-breakfast specimen, react. acid; sp. gr. 1025; albumen absent, also uric acid and oxalate of lime. Evening specimen, react. highly acid; sp. gr. 1034; albumen, both by heat and nitric acid; urates precipitated by nitric acid; acidulated, boiled, and allowed to stand, albumen  $\frac{1}{2}$ . Microscopic examination: very numerous oxalate of lime crystals.

The patient had discontinued her medicine, and had taken little exercise during the previous week. She had, moreover, been suffering from much mental anxiety. Greater care in diet and more vigorous out-door exercise were urged and adopted, and on the following examination, Dec. 10th, a week later, the urine was found to be entirely normal. The points of especial interest in the above case are the transient character of the albuminuria, its invariable appearance and disappearance in the presence and absence of uric acid and oxalate of lime. The patient has not been under my observation for a sufficiently long period to express a positive opinion in regard to the absence of structural kidney change, but the symptoms certainly very closely resemble those observed in the group which I have described.

CASE 5.—Ethel M., aged 4 years. Has never had diphtheria or scarlet fever. The urine was examined on account of advice being sought for incontinence of water, the patient being in excellent health at the time. It was found to be markedly acid, of

high sp. gr., to contain albumen  $\frac{1}{4}$ , and uric acid with oxalate of lime in large amount. Two days later the urine was free from albumen and the above salts. The patient has been under my observation up to the present date, three years from the first examination; her health has been unusually good, and in frequent examinations (last date Nov. 12, 1881) the urine has been invariably normal.

CASE 6.—Ethel W., aged 9 years. Had scarlet fever without albuminuria when 4 years old. Has never had diphtheria. The patient has a neurotic family history. In July, 1880, had a convulsive attack, which was attributed to indigestion. Subsequent attacks demonstrated their epileptic nature. Under treatment further paroxysms have not occurred. The record of an examination of the water, Oct. 8, 1880, is as follows: Evening specimen, react. acid; sp. gr. 1031; distinct amount of albumen both by heat and nitric acid. Microscopic examination: a large amount of uric acid present.

An examination of an after-breakfast specimen of the same date gave similar results, differing only in having a lower sp. gr. Examination a few days later, morning specimen: react. acid; sp. gr. 1014; free from albumen; microscopic examination, negative. Evening water, same date: react. acid; sp. gr. 1026; *trace* of albumen, with *few* uric acid crystals present. On the following examination, although the sp. gr. of the urine was 1033, it was wholly free from uric acid and oxalate of lime, and did not contain a trace of albumen.

Very frequent examinations have been made up to the present date, and the records show the invariable absence of any trace of albumen; also the absence of uric acid and oxalate of lime. It may be mentioned that the diet has been most strictly regulated during this period, and careful attention given to all other hygienic measures.

CASE 7.—Florence T., aged 4 years. The patient has a lithæmic diathesis. Has never had diphtheria or scarlet fever. During the winter of 1880 advice was sought for an attack of urticaria. The tongue was coated, the bowels disordered, and the cutaneous symptoms very pronounced. There was no fever. Examination of the urine: react. acid; sp. gr. not recorded; a considerable amount of albumen both by heat and nitric acid; urates precipitated by the addition of acid; microscopic examination, amorphous urates.

The following examinations gave entirely negative results. The

child has continued to be in excellent health up to the present date.

Urticaria is now universally ranked among the neuroses, and it is a neurosis in which the vaso-motor system is pre-eminently affected.

Moreover, a temporary lithæmic state is recognized as a frequent factor in its genesis. The above case may therefore not unreasonably be regarded as lending support to the theory of transient albuminuria occurring in apparent health, which has been suggested in this paper. I possess the notes of two other cases of temporary albuminuria in children of 4 and 5 years of age respectively, associated with transient lithæmia, but as they closely resemble the cases already narrated, their recital is unnecessary. I have at present under observation four additional cases in adolescents, but as they have only recently come under my notice, I do not feel warranted as yet in including them among the above.

Inasmuch as it has been wisely said "that much of that which each man thinks that he observes is but a part of himself," it may be mentioned that Dr. Draper kindly consented, at my request, to repeat the examination of the urine in many of the cases which I have recorded, with the result of corroborating my observations.

The presence of uric acid, either in the crystalline or amorphous form, even in large quantities, in a concentrated and highly acid urine, cannot be accepted as a demonstration of its excessive elimination from the body. Oxalate of lime, however, is present in the normal urine only in minute quantity (Schultzen<sup>1</sup>) if at all (Neubauer<sup>2</sup>); urine, therefore, found to contain a large amount of oxalate of lime within two or three hours after its passage, may fairly

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<sup>1</sup> *Archiv f. Anat. und Physiol.*, 1868, p. 719, *et seq.*

<sup>2</sup> "Analysis of the Urine," Neubauer and Vogel, p. 168.

be judged to indicate an excessive elimination of this imperfectly oxidized nitrogenous matter.

In my own cases the urine in several instances was examined two hours after being passed, and showed the presence of a large quantity of oxalate of lime crystals alone.

Moreover, in the majority of all the cases the constitutional symptoms were of a character usually present in lithæmic patients.

The very constant association of imperfectly oxidized nitrogenous matter in the urine and albuminuria, especially in Dr. Moxon's and my own cases, cannot reasonably, it seems to me, be regarded as a simple coincidence; in the latter observations we find an additional argument against such an hypothesis in the disappearance of the albuminuria *pari passu* with the lithæmia. Dr. Murchison<sup>1</sup> maintained that such an association, at a later period in life, not only was to be regarded in the light of a causal relation, but he also agreed with Dr. George Johnson in believing that "renal degeneration may be a consequence of the long-continued elimination of products of faulty digestion through the kidneys."<sup>2</sup> That temporary albuminuria in children and adolescents in apparent health is invariably dependent upon a transient oxaluria or lithuria, I do not maintain. The observations of Leube, Ultzmann, Fuerbringer,<sup>3</sup> and others would seem to show that prolonged and vigorous physical exercise is capable of occasionally producing, though in an as yet unsatisfactorily explained manner, a slight transient albuminuria in presumably healthy persons. I have not referred to the theory of chemical or "food albuminuria" in explanation of the condition which is the subject of this paper, for the reason that careful investigations and experiments leave it extremely doubtful whether albuminuria (the presence in

<sup>1</sup> "Clinical Lectures on Diseases of the Liver," Amer. edition, pp. 572, 573.

<sup>2</sup> *Brit. Med. Journ.*, 1872, vol. I, pp. 161-191.

<sup>3</sup> *Loc. cit.*

the urine of a body coagulated by heat or precipitated by neutralization—Saundby) is ever due to the transudation of a modified or more easily diffusible form of albumen. Dr. Saundby has shown, by a series of very interesting observations and experiments,<sup>1</sup> that the several tests, viz., an increase of albumen after food, a greater diffusibility of such albumen (Parkes, Pavy), and a difference in the coagulation temperature (Lauder Brunton and D'Arcy Power), which have been used by different investigators in their attempt to prove this point, are not to be relied upon.

In conclusion, my observations would seem to show that temporary albuminuria, as it occurs in children and adolescents in apparent health, may be traced in a large number of instances to a transient oxaluria or lithuria, and I would suggest that the sequence of events in the causation of the albuminuria is as follows:

1. The temporary presence of a large amount of imperfectly oxidized matter in the circulation.
2. A disturbance of the general nervous system, in which the vaso-motor system of the kidney shares, or one confined to the vaso-motor system of the kidney in its elimination of these products of a faulty digestion.
3. A transient dilatation of the blood-vessels of the kidney and a retardation of the blood current in the glomerular vessels, with a consequent possible alteration in the functions of the glomerular epithelium, also of a temporary nature.

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<sup>1</sup> *Brit. Med. Journ.*, vol. 1, 1880, p. 841; *Birmingham Med. Review*, July, 1879.

## NOTE ON A CASE OF LOCALIZED CEREBRAL ATROPHY.

BY J. C. SHAW, M.D.,

MEDICAL SUPERINTENDENT OF THE KINGS COUNTY LUNATIC ASYLUM, BROOKLYN, N. Y.

THE physiological experiments of Hitzig, Ferrier, Carville and Duret, Munk, and many others, have tended to show that in the cerebral cortex there are more or less localized motor and sensory centres; a large quantity of pathological material has been collected in support of these physiological deductions by Charcot and his pupils Ferrier, Seguin, De Boyer, and many others.

The largest part of this material collected is in support of the doctrine of localization of motor centres, and up to this time our knowledge of the sensory centres, physiologically and pathologically, is quite limited.

With regard to these sensory centres Ferrier says: "When the lesion is accurately circumscribed in the angular gyrus the loss of vision is the only effect observed, all the other senses and the powers of voluntary motion remaining unaffected."

He then details an experiment in which the angular gyrus was injured, and says: "The animal retained complete muscular power and every other form of sensation, except sight, the conditions as regards which being repeatedly tested in various ways." And he further says: "Destruction of the angular gyrus on one side causes blindness in the opposite eye; the loss of vision is complete but

is not permanent if the angular gyrus of the opposite hemisphere remains intact, compensation rapidly taking place, so that vision is again possible with either eye as before. On destruction of the angular gyrus in both hemispheres, however, the loss of vision is complete and permanent, so long at least as it is possible to maintain the animal under observation." Ferrier, therefore, concludes that the centre for vision is in the angular gyrus. After detailing some experimental injuries of the superior temporo-sphenoidal convolution, he says :

"When the lesion was established bilaterally so as to cause destruction of the temporo-sphenoidal convolution on both sides, along with certain other effects not depending on localized injury of this convolution, the animal, though fully conscious and on the alert to every thing attracting sight, failed to respond to auditory stimuli usually exciting active reaction and attention."

He, therefore, places the auditory centre in the superior temporo-sphenoidal convolution.

Munk finds that extirpation of a particular part of the occipital lobe in dogs causes psychic blindness; that is, the memory for visual impressions previously received is abolished, and after extirpation of the two occipital lobes in monkeys they become completely blind, they are unable "to see any thing."

Ferrier<sup>1</sup> gives no pathological cases in support of his physiological localization of the centres of hearing and vision.

De Boyer<sup>2</sup> says, in regard to this subject, that "the pathological facts yet reported are not sufficiently numerous, and are too dissimilar in character for any deductions to be made from them."

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<sup>1</sup> Ferrier: *The Localization of Cerebral Disease.* New York, 1879, lecture iii.

<sup>2</sup> De Boyer: *Etudes Cliniques sur les Lésions Corticales des Hémisphères Cérébraux.*

The case now reported is not strong evidence of this localization of centres of hearing and vision, but is remarkable in that the localized atrophy found occupies the region in both hemispheres in which these centres have been located by Ferrier; the case also has some interest in regard to the localization of motor function in the cortex.

A case has just been reported by Dr. Chauffard<sup>1</sup> of what he entitles cerebral blindness and deafness.

Male, aged 44, without serious disease up to the beginning of this illness; two or three months ago began to have œdema of lower extremities and abdomen; at present he has marked œdema of lower extremities, and ascites; urine scanty and contains albumen; face cyanosed; no valvular disease of heart discovered. Oct. 10th, in the evening, without preceding symptoms and without loss of consciousness, the patient loses at once his speech and the use of his intellectual faculties, and the next morning we find him in the most curious cerebral state; remains seated on his bed in the posture habitual to him, and the only one which the size of his abdomen permits; supported on his two elbows, he remains immobile, eyes fixed, lost in vague and apparently profound meditation; he is spoken to, shaken by the shoulder, the hand is brought quickly before his eyes, he sees nothing, he hears nothing, he appears to be unconscious of what is going on around him; if he is spoken to loudly and sharply he, nevertheless, turns his head to the side from which comes the sound, but without permitting the belief that he comprehends what is said to him,—that the auditory impression which he has vaguely received has awakened in him an idea or a memory.

The same for vision: the pupils are partly dilated and immobile, the eye vague, it follows not the objects which pass in the visual field, and, nevertheless, the patient is not properly speaking amaurotic.

Besides these curious troubles the patient still preserves the use of his motor functions.

Nov. 12th.—Same state of absolute cerebral unconsciousness; the patient appears to be a perfect stranger to all that surrounds him. He remains silent, muttering, half audibly, incoherent and

<sup>1</sup> Chauffard: Note sur un Cas de Cécité et Surdité Cérébrales; *Revue de Médecine*, No. 11, November, 1881.

unintelligible words, or repeating, ten times an hour, mechanically and with the same monotonous intonation, "I am better, I am better."

Ophthalmoscopic examination shows no lesion at fundus oculi. Patient died Nov. 13th, and autopsy shows integrity of meninges on both sides. Brain normal, except left hemisphere, and it contains one lesion of considerable extent, visible at first sight and perfectly isolated. It is a focus of red softening of circular form, about the size of a five-franc piece, and occupies *le lobule du pli courbe et le pli courbe*. Above, it reaches the interparietal fissure without passing it ; below, it extends over the superior extremity of the temporo-sphenoidal fissure (*scissure parallèle*) and the two convolutions which limit it, first and second temporal. This lesion occupies the entire thickness of the gray substance of the convolutions, impinging even very lightly on the white matter.

PERSONAL CASE.—M. R., aged 34, admitted to asylum Sept. 25, 1879; married ; has had four children, the last fifteen months ago. The husband of the patient is a very ignorant and drunken man, and can give no history of his wife's condition. The following is obtained from her sister : Two months before admission she complained of being unable to use her right upper extremity. She could not lift any thing with it ; thinks she had no trouble with the right lower extremity. Soon after she had a sudden attack, characterized by sudden loss of consciousness, inability to speak, and loss of hearing. They did not notice at this time that she had any loss of vision. She remained unconscious two weeks (?), after which time she was able to get out of bed, and then began to talk, but she would talk sensibly only for a few minutes ; then she would ramble off and become incoherent. It would appear that at this time patient must have had trouble with her vision, although the friends are quite indefinite about it, and she evidently at this time had marked mental disturbance. Patient told her sister that the sudden attack was preceded by a pain in right ear, then pain in left ear and side of head, and she knew no more. She has had no convulsion since the first attack up to this time. It is said that she was paralyzed on the right side after this attack, but it soon passed off.

The certificates committing her, relate that she says some men are trying to kill her, and she screams without cause, and is at times violent. On admission patient is very stupid ; does not answer any questions, evidently does not hear them ; is quite deaf ; pupils slightly dilated and regular. She is in poor physical condition :

scars on face, evidently quite blind, bowels constipated, digestion poor, pulse feeble.

Oct. . . . —It is noted that she soils her clothing and bedding, and goes groping about the hall, talking disconnectedly and at times screaming out.

Oct. 18th.—Had a severe epileptiform fit, which lasted about three hours; the muscular spasm began on right side, and became general; cerebral tumor is suspected.

Oct. 21st.—Was very stupid after the convulsion; to-day has improved, but is quite restless; tosses about and screams aloud as if in pain.

Nov. 6th.—Still remains very heavy and stupid; sleeps the greater part of the time.

Nov. 21st.—Has been in bed without special change most of the time since last note; seldom sits up; to-day had a convulsion, with marked spasm of left side of face; stertorous breathing; limbs relaxed.

Dec. 2d.—Slowly recovering, but is still very stupid from last fit.

Dec. 11th.—Repeated tests of tactile sensibility and smell show them to be intact, and repeated testing shows her to be absolutely deaf to all noises. All the other cranial nerves appear normal in function.

Dec. 16th.—Walks about the hall, using disconnected and unintelligible words; at times speaking very loudly, and screaming.

Dec. 22d.—From the way in which she uses her right hand it is inferred that she has slight paresis of that extremity; it is impossible to apply the dynamometer, as she is so very stupid, blind, deaf, and screams and resists when interfered with; for the same reason no ophthalmoscopic examination has been obtained; she presses her hands to her head as if in pain; at night is very noisy.

Jan. 31, 1880.—Had a severe convulsion, which lasted from 10 A. M. to 5 P. M.; face very much congested; pupils dilated; profuse salivation; muscular spasm, most marked on right side. At 12 M. (midnight) was quieter; T.  $96^{\circ}$  in axilla; at 2 A. M., T.  $98^{\circ}$  F.

Feb. 1st.—At 10 A. M., T.  $100\frac{1}{2}^{\circ}$ ; P. 100; 7 P. M., T.  $101^{\circ}$ ; P. 108; lies all drawn up in bed.

Feb. 2d.—10 A. M., T.  $99^{\circ}$ ; P. 80; 7 P. M., T.  $98\frac{1}{2}^{\circ}$ ; P. 76; muscles of right arm quite firmly contracted.

Feb. 6th.—Patient uses right arm a little this afternoon.

Feb. 14th.—Is up again; walks about; shows no sign of paresis anywhere.

Oct., 1880.—She has become quite noisy, talking and screaming unintelligibly night and day; is growing thinner and more stupid, if that were possible.

Patient remains in about the same condition until Sept., 1881, when she develops a low form of pneumonia, and dies Sept. 19, 1881.

*Post mortem Examination.*—Only the head examined; dura to naked eye appears normal; nothing special about skull and sinuses; slight meningitis everywhere, but it is not marked. There is a remarkable atrophy of the superior and inferior parietal convolutions on both hemispheres; the atrophy is so marked and localized that it presents a striking contrast with the other portions of the brain. The cranial nerves present a normal appearance to the naked eye; the atrophied convolutions are quite firm to the touch; there is no atheroma of vessels.

The brain is hardened in bichromate of potassa, further examination of which shows that the upper part of the ascending parietal on the left side is very slightly atrophied, and the same convolution on the right side is also very slightly atrophied at its lower portion. This atrophy is in a very limited part of the convolutions.

To give some idea of the degree of atrophy, we will take as a comparison the first temporo-sphenoidal convolution in a brain hardened in bichromate of potassa and without special wasting. A rough measurement shows it to be about  $\frac{3}{4}$  to 1 inch broad, and in the same convolution of our patient it is  $\frac{1}{2}$  inch, and less in some places.

During the life of this patient it was at first supposed that she might have cerebral tumor, but this did not explain the association of absolute blindness and deafness without the involvement of other cranial nerves.

It was suggested by Dr. Seguin that it was possibly meningitis, which at first appeared to be the true pathological

diagnosis, but on consideration it appeared improbable that a meningitis should disturb so completely the functions of the auditory and optic areas and leave all the others intact, and in the absence of a more complete history, it appeared impossible to arrive at a satisfactory pathological diagnosis. On *post mortem* examination the atrophy was found so strictly localized to the convolutions in which have been located the centres of vision and hearing by Ferrier, that the question at once arose, is it possible that this is a case of absolute blindness and deafness due to cortical lesion? and it appeared a very fascinating view of the case.

Microscopic examination was made of the atrophied convolutions, and it was found that the gray matter had entirely disappeared, leaving the very outer layer, to which the pia was attached, and the white matter below, so that there was really a cavity, or space, between the two, formed at the expense of the gray matter. Microscopic examination showed this outer layer to be composed of dense connective tissue filled with nuclei.

The white matter under it contained an enormous quantity of nuclei of all sizes; the perivascular spaces were very much dilated; and from the vessels to the wall of the perivascular space many bands or fibres of connective tissue ran, and in which were entangled hematoïdin crystals; the walls of the perivascular spaces and the tissue immediately around them were the seat of a large quantity of closely placed nuclei; the blood-vessels appeared normal.

Examination of the optic nerve microscopically shows a more or less general increase of the connective-tissue septa running between the nerve bundles; the nerve fibres themselves are atrophied; and there are numerous small areas of disintegration of nerve fibres, the spaces being filled with a colloid-looking (?) material, which is faintly stained with hematoxylin: this is a similar appearance to that seen

in spinal cords which have undergone degenerative processes.

What relation do these pathological conditions bear to the symptomatology in this case?

The atrophy of the angular gyri and the first temporo-sphenoidal convolutions, the supposed centres for vision and hearing, tempts one to assert that in this is to be found



FIG. 1.—Distribution of cortical atrophy shown upon an Ecker's diagram: right hemisphere.

the explanation of the blindness and deafness, the association of which, without involvement of other nerves, was so difficult of explanation during life; but the microscopic examination of the optic nerves deters me from making this as a positive statement, as a second question arises: Are the appearances found in the optic nerves due to primary lesion in them, or are they degenerative, and due to the

destruction of the cortical visual centre? It appears to me *possible* that these changes are degenerative, and due to the lesion of the cortex, but no more definite answer can be given.

It has been noted that the patient, from her movements, appeared to have some paresis of the right upper extremity, which passed off; and at another time it is noted that she



FIG. 2.—Distribution of cortical atrophy on the left hemisphere.

had marked rigidity for several days of the right arm, which also passed off. These passing motor disturbances appear to be explained by the proximity of the lesions found to the motor district—the ascending frontal and parietal convolutions,—especially as there is a small place on each hemisphere where the ascending parietal convolution is visibly wasted. It is of interest to note, however, that these motor

difficulties were passing, not permanent; and this shows that lesions, in the brain particularly, may be so slight, and at a particular moment they may cause more or less grave motor disorders—paresis and convulsions even,—which are passing; and that the lesions must be of considerable extent and severity to cause permanent motor disorders.

The absence of accurate information as to the state of vision at the onset of the attack in our patient is to be regretted; the sudden loss of hearing appears to have been made out pretty clearly. This case resembles in many respects the one reported by Chauffard: in his case the lesion was in one hemisphere, and there were absolute blindness and deafness; in our case the lesion was in both hemispheres, and in that respect more in keeping with Ferrier's physiological experiments; the mental condition of our patient appears to have been about the same as in Chauffard's.

## EDITORIAL DEPARTMENT.

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### SPECIALISM IN MEDICINE.

We propose to consider briefly, but critically, the following proposition, which, though not distinctly formulated, is, as it were, held in solution in many others now current, and may be easily precipitated from them.

At the present day medical science has expanded to such an extent that its intelligent cultivation as a whole by any one person has become impossible. The practice of medicine, therefore, to the extent to which it may reach any really high standard of excellence, must henceforth be carried on exclusively by specialists.<sup>1</sup>

Thus, the physician, who should, in chimerical imitation of Lord Bacon, propose to "take all (medical) knowledge for his portion," must, on this theory, be consigned to a limbo of worn-out inanities. Nevertheless, the most useful functions of specialists are still exercised with tacit reference to the intelligent practitioner, who is compelled, not indeed to know all about all medicine, but to hold the key of admission to any of its branches, of which, at any moment, he may have practical need.

Thus, specialists are justly expected to become the depositories of special literature, and to so sift, handle, classify, and arrange

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<sup>1</sup> \* \* \* "The fact, the hard and undeniable fact, that all intelligent and scientific physicians are quasi-specialists, and must be. In the present development of medical science there is no alternative; a physician must be a quasi-specialist, or possess a universal knowledge of a superficial, mostly booky kind,—a knowledge wholly insufficient to insure intelligent or successful practice."—E. C. Seguin, these ARCHIVES, April, 1881, p. 186.

this, that it become accessible to, and utilizable by the general practitioner. By reiterated experience, they are expected to acquire an exceptional familiarity with certain types of disease, so as to be better able to decide in rare, obscure, or unusually difficult cases, when the physician shall call them in. By continued application they may tend to indefinite improvement in the technique of diagnosis and of treatment. Finally, in regard to the state of medical knowledge on any given question at a given moment, they may furnish the standards with which the knowledge and practice of the general physician must constantly be compared and tested. Thus, specialism is largely useful in furnishing the *exact* material with which the general physician may make his practical combinations. In his absence, and from the languid interest which specialists profess in each other's departments, this combination would often not be effected. But the problem offered by a sick person is always a problem of combination. The practical specialist does not analyze, but roughly divides this problem according to considerations frequently artificial. The scientific specialist abstracts phenomena completely; studies separately, anatomical, physiological, chemical, pathological conditions. It is the ideal business of the physician to take conditions which science has abstracted for the purpose of thought, and to recombine them for the purposes of life. In the absence of the physician there would be no one to do this; with every new deterioration of the ideal character of the general physician, this work of combination is less and less well done. As a consequence, every sick person who can pay for it begins to expect to divide up his body among a cluster of "eminent specialists" before any positive diagnosis of his case can be reached.

Notwithstanding the inconvenience and expense of this procedure, it tends to gain in popularity on account of the simplicity and apparent common-sense of its theory. The laity are very ready to infer not only that specialism is good, but that the more of it the better. If the physician who treats six diseases is necessarily superior to him who is willing to manage sixty, then he who confines himself to one must be the best of all. Hence the

popularity of the pile doctor, and the cancer doctor, *et hoc genus omne.*

The great principle of unity in diversity, whose research is the problem of philosophy, is also the animating principle of philosophical medicine. But this cannot be appreciated by persons who are neither physicians nor philosophers.

The complete theory of practical specialism admits that a man may be a shining light in a subject "which interests him," yet a perfect idiot in another of equal importance to the patient. Now, the initial problem of diagnosis is the decision of the department to which the case belongs ; and, on the above theory, the fate of the patient must be a matter of chance. If his case happen to fall on the competent side of the doctor he consults, well and good ; but if not, it must fail of recognition. No fixed value can be attached to any symptom, when it is remembered that the lines of disease intersect each other in every direction.

Thus, does a young girl fall into a melancholy ? The question would arise : Shall she be at once entrusted to the gynecologist on the suspicion of uterine disease, or to a haematalogist for chloro-anæmia, or to the superintendent of an asylum as a case of incipient insanity, or to a friend of the family to bring about a thwarted project of marriage ? If a woman has a pain in her back, how many physicians must be consulted before deciding whether this be due to muscular denutrition, or to uterine displacement, or to chronic nephritis, or incipient myelitis, or to commencing caries of the vertebra, or merely to hysteria ? When a typhoid fever simulates general tuberculosis, or the reverse, should the diagnosis be made by the heart and lung specialist, or by the fever doctor ? When a man falls down in an apoplexy, does his case belong to the neurologist, or to the specialist in diseases of the heart whence an embolus may have been carried, or to the practitioner devoted to gout and atheroma ? Shall a children's doctor decline to perform an urgent tracheotomy because he is not a surgeon ? or shall a physician tolerate irreparable delay in reducing a dislocation for the same reason ?<sup>1</sup>

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<sup>1</sup> We have within a few weeks seen two cases of irreparable injury caused by

It is sometimes said that the conscientious specialist will be sufficiently trained in general pathology to recognize when a subject lies beyond his domain, and he will then, "in justice to his patient," hand him over to one of his own "eminent colleagues."

Dr. Barnes, who, of all gynecological specialists, most frequently deprecates specialism, thus illustrates the case: "A woman comes to him complaining of pruritus. Much to her astonishment, he examines her urine, because he retains enough knowledge of general pathology to know that pruritus may indicate diabetes. Finding sugar, he at once resigns the case and sends her elsewhere." This illustration represents a class of cases which do often occur, and where the specialist is really both competent and conscientious the case may be managed without further inconvenience to the patient than that of a double consultation. But—and this is a practical inconvenience of perhaps a low order for mention here—there is certainly no more, but rather less, guarantee for the honor of a specialist than of a general practitioner. The last is expected to take charge of the patient whatever may prove to be the matter with him. His interest, therefore, in ascertaining the exact state of things is identical with that of the patient. But the specialist knows he will only be entrusted with the case if he can prove that it falls within the limits of his own specialty. He is therefore always under a strong temptation to "make out a case," and for this purpose, if necessary, to rather avoid than to seek close scrutiny of the surroundings.

We hasten to recognize the fact that there are many specialists of honor as high and unsullied as could be claimed for the most upright physician. But we think the existence of the special temptation we have referred to can hardly be doubted, nor that this temptation is by no means always resisted. Apart from this purely practical consideration, it is to be remembered that such definite grounds of classification are more often absent than present; the specialist confronts the theoretical difficulty of not being quite sure what he is to exclude.

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just this fact, and by the prolonged application of poultices instead of prompt operative interference.

Another important inconvenience in the tendency to universal specialism is that the beginnings of disease are so often likely to escape detection. To consult a specialist, the patient will first wait until he is pretty sure he has the specialist's disease ; thus, he must wait until this is rather well developed. Thus, too often no attempt is made to treat a chronic disease until it has become almost incurable, nor to make the precise diagnosis of an acute disorder until it has nearly killed the patient.

But the collapse into inefficiency of a general practitioner is not an adequate basis upon which to develop an accomplished specialist. Instead of either the one or the other, we obtain a confused, vague, cheerfully optimistic "family doctor," who relieves himself of responsibility for one organ in his patient's body after another on the ground that it belongs to some "specialist," who, as long as symptoms are not importunate, declares that they will "pass away of themselves,"—instinctively dreading the recognition of their importance as the signal for a surrender of the case. Thus, epitheliomas are allowed to extend until they are ineradicable, and chronic pneumonia to eat out caverns in lung tissue unsuspected, and the child to limp from habit into a suppurating coxitis, and the melancholic to commit suicide while sent on a journey for change of scene.

In addition to the functions which may be unquestionably fulfilled by specialists with great advantage to the community at large, other claims are often advanced of, we believe, less validity. Thus, it is said :

1. That to specialists alone, or chiefly, is due not only the improvement of technique, but the discovery of the fundamental ideas which change the face of science.
2. That specialists are habitually engaged in life-long researches in the subjects of their specialty.
3. That, thus, the patients of a specialist must profit much more by his intellectual activity than can the patients of a general practitioner by his.
4. That, whereas a general practitioner can only have at best a partial acquaintance with the many diseases he treats, the special-

ist, in virtue of his wise limitation of observation, can know all about his.

5. Finally, that the establishment of specialties alone permits the accumulation of clinical material in definite and available masses.

The first claim might be contested *a priori* from the consideration of the evident necessities of the case. No idea in a specialty can be as fundamental or as original as that on which the specialty is founded, and this evidently must have been suggested by a non-specialist. Laennec was not a specialist when he practically discovered the principles of auscultation; his prolonged special application afterward was devoted to the consolidation and simplification and detailed establishment of his theory. Helmholtz was no oculist when he invented the ophthalmoscope; even his treatise on optics was written later. Czermak was not a specialist when he invented the laryngoscope. Orthopedics, perhaps, dates its modern impulse from the researches in locomotion of the brothers Weber, who were physiologists. The principle of counter-irritation in joint diseases was established by Pott, a general surgeon of London; the still more important principle of rest was elaborated by Bonnet, a general surgeon of Lyon. The effective introduction into orthopedic surgery of resection was made by Sayre before he became an orthopedist. In gynecology the capital operation of ovariotomy was initiated, as is well known, by McDowell, a general surgeon, having been originally suggested by Hunter, than whom none of the great physicians of the eighteenth century was less of a specialist. It was the great surgeon Velpeau, and the author of a treatise on neuralgia, Valleix, who first called attention to uterine flexions and suggested pessaries. Dr. Sims had hardly become a specialist when he invented his speculum and contrived his operation for vesico-vaginal fistula, achievements which his long career has never enabled him to excel.

Modern dermatology is based upon anatomical researches, which may be, and often are, carried on by histologists who do not practise medicine at all,—hence could not be called practising specialists. The clinical researches of the French school, being con-

ducted according to the theory of diathesis, were not and could not be made by physicians limited in clinical observations of skin diseases. The theory may be discarded ; but the results of the impulse given under its influence remain. In neurology clinical specialism was first suggested by anatomy, and later by physiology. In no practical specialty is modern clinical observation kept more closely to these two fundamental sciences than in this. The principal facts and ideas have come from anatomists or physiologists, or from non-specialists, who have also furnished the chief clinical groupings. Bell's discovery of the double function of the roots of nerves was made in his capacity of anatomist ; his discovery of external facial paralysis, in his capacity of general practitioner. Marshall Hall, Brodie, Abercrombie, Calmeil—even Broussais, with his "*De l' Irritation et de la Folie*,"—and a host of others, who were the early pioneers in this century in the study of nervous diseases, were not specialists, since it was indeed at that time not possible to be one. Nevertheless, many of their observations remain of permanent and fundamental value. The most eminent physiologists, who have contributed to knowledge of nervous diseases far more than have simple clinicians, have not been specialists in the physiology of the nervous system. Magendie, who divides with Bell the honor of the discoveries in the spinal roots of nerves, wrote two volumes on the "*Physics of the Animal Organism*." Bernard is as distinguished for his composite researches in diabetes (to go no further) as for those on the vaso-motor system. Schiff, who distinguished the paths in the cord for different sensory impressions, has written a treatise on digestion. Neither Türck nor Bouchard were practical specialists when they established the fact of descending degenerations ; nor was Waller when he made the famous experiment which has served to explain these morbid processes. Brown-Séquard's researches in epilepsy were made at the very beginning of his career, and not when he had become a specialist. The clinical groups of locomotor ataxia and pseudo-hypertrophic paralysis were established by Duchenne, whose specialty was not nervous diseases, but faradic electricity, and originally, in its application to orthopedics. Ex-

ophthalmic goitre has been discovered by Basedow, a sagacious general practitioner; and the same is true of Addison's disease. Gubler, the first to point out crossed paralysis, was never a specialist; indeed, his essay on the hepatic lesions of hereditary syphilis is as famous as any that he has written. Sir William Gull's and Stanley's observations on paraplegia from renal calculus initiated research into "reflex paraplegia." No one could suppose them to be specialists.

Another class of examples is offered by writers who had become specially identified with neurological practice before publishing the treatises now recognized as authoritative, yet who, before this, had achieved distinction in other directions. Thus, Griesinger's now classical work on psychiatry was preceded by an only less famous treatise on infectious diseases. Leyden, before writing two volumes on diseases of the spinal cord, had published a valuable monograph on icterus. Nothnagel's admirable clinical contributions to the problem of cerebral localization, and his less admirable experiments on the brain, cannot efface recollection of his hand-book of therapeutics—on the whole, the most valuable extant on the subject. Charcot began his studies in neurology by general studies on the diseases of old age. He was stimulated by the practice of no specialty, but simply utilized the neglected pathological materials accummulating in oblivion at the Salpêtrière. Only recently, moreover, Charcot has published a series of lectures on the pathology of the liver and of the kidney; and his description and analysis of the lesions of broncho-pneumonia have thrown new light on a subject supposed to have become hackneyed.

These examples, selected at random, do not of course exclude the clinical discoveries or inventions which have been made by practising specialists, and in a manner which indicates that they were the direct outgrowth of their special clinical experience. In neurology, Westphal's discovery of the tendon reflex symptom; in gynecology, Emmet's operation for lacerated cervix, are typical examples of this class. The fact that Hitzig, whose discoveries on the motor irritability of the cortex have had such an enormous

influence, has been for a long time the superintendent of an insane asylum, is not an example of the influence of practical specialism. His researches were purely physiological, and were suggested by physiological considerations, which clinical observations might confirm, but did not suffice to originate.

We think the cases quoted are sufficient to demonstrate that indefinite repetition of clinical experience is never of itself sufficient to suggest new ideas; that a life-long specialism in no wise predisposes to discoveries, and still less is essential to their achievement; that in a large number of cases, if not the majority, the consecration to a specialty has followed, and not preceded, the discovery which has achieved the reputation of the specialist, and has fascinated him, perhaps for ever, with the subject. But it is always genius which invents; special application can only improve; it then remains for culture to appropriate.

Our limits compel us to be brief with the three remaining propositions. In regard to the second claim, namely, the life-long researches supposed to be carried on by practising specialists, we would call attention to a fact usually overlooked. It is that for every mind, in regard to every subject it studies, there exists a saturation point of suggestiveness, which is not exceeded by enforced prolongations of attention. It is very useful for a person to pursue a subject, so long as it continues to yield him ideas; very useful to practise a technique, until it be sufficiently mastered to meet all difficulties of execution. But afterward there remains no intellectual advantage in persistent adherence to the same line of thought. There are personal, often pecuniary advantages; there is profit gained from an acquired reputation and previous labors. But this, however legitimate, is a very different thing from continued progress in science, or indefinite improvement in care-taking of patients, such as is generally assumed.

Again, the practical specialist does not, fortunately, often select only one disease, but one organ, or presumably associated group of organs. Now cases of the same disease in different organs are apt to present many more points of resemblance than do cases of different diseases in the same organ. There is much more analogy

between uterine cancer and epithelioma of the lip than between uterine cancer and uterine flexions. The study of the pelvic curves throws no light on embryology, although both subjects are assigned to the obstetrician. Uræmic peritonitis is better understood by study of septic peritonitis than of renal calculus. Epilepsy has much less resemblance to the systemic forms of myelitis than to the eclampsia induced by acute hemorrhages, and so on.

Practical specialism only enforces attention to clinical observation : analysis of this, on the basis of any special science, is as optional with the specialist as with the general practitioner, and as liable to be neglected. Many good specialists are purely clinicians ; many others, really distinguished in some branch of science connected with special disease, are quite innocent of others. Perhaps from few experts in consultation would we expect familiarity with such a monograph as Bert's on respiration, or with the complex laws on diffusion of gases. It would not be difficult to name neurologists distinguished in experimentation, but who have never mounted a section of nerve tissue for the microscope. It would not be impossible to cite skilful surgeons, most ingenious in mechanical contrivance, who are unaware of the pathological anatomy of the tissues they divide or remove.

Great as are the difficulties arising from the great increase in the mass of knowledge, there are many palliations. The perfected machinery for sifting, analyzing, classifying, and sorting this knowledge, renders it ten times as accessible and comprehensible as was formerly one tenth part as much. Many general principles have been established, which link together, in lucid unity, hosts of details, once unconnected, unintelligible, and hence most difficult to remember. The classical body of doctrine in medicine, whose possession is essential to the practice of medicine (*secundum artem*), is really more accessible to-day than at epochs when some narrow system professed to crush it into a portable nutshell. Finally, the advance of science and of scientific method exacts, that who would claim to contribute to further progress must concentrate himself much within the limits of any conventional specialty. No one disease, no one organ may be compassed by a

single observer: happy he who may, by laborious research, contribute to the solid establishment of a single detail of the truth. For such work it is, theoretically at least, as easy for the general, as for the special physician to withdraw a certain portion of his attention from practice. Neither can hope that his research can benefit more than a small proportion, if any, of his own patients. The one must, as much as the other, depend on the collaboration and unconscious coöperation of a thousand workers. For both, are needed not only clinical observations, but the mental ability to utilize observations,—a mental training in the art of handling large masses of ideas. For both, if we may judge from European examples, the personal experience to be gained in private practice is insufficient; to both, should classified hospitals be open as the true field for pathological study.

## NEW BOOKS AND INSTRUMENTS.

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### **Transactions of the American Gynecological Society.**

Volume v. For the year 1880. Boston : Houghton, Mifflin & Co., the Riverside Press, Cambridge, 1881. 8vo, pp. 470.

This handsome volume, which typography, paper, and binding combine to render a pleasure to the eye, contains the year's record of an association which has already obtained a world-wide repute. Thanks to the achievements and incomparable energy of a few of its leading members, American gynecology stands to-day confessedly unrivalled as a surgical specialty. No association or society represents it except the one named in the title to this volume, nor has it any other means of bringing its views before the public. Hence, those who look to this body for instruction, new methods, and encouragement have a right to hope that their aspirations will be gratified.

Does the volume before us meet such expectations? An examination of its contents will furnish the answer.

The volume contains :

1. List of officers for 1880.
2. List of honorary fellows.
3. List of active fellows.
4. Minutes of the proceedings of the fifth annual meeting.
5. Papers read at the fifth annual meeting, consisting of the annual address by the President, and fourteen papers on various obstetric and gynecological subjects.
6. Three indices, elaborately and carefully prepared :
  - (a) Of obstetric and gynecological journals.
  - (b) Of obstetric and gynecological societies.
  - (c) Of gynecological and obstetric literature for the year 1879.

The President, Dr. Sims, deals in his annual address chiefly

with the progress of the Society, or rather its lack of progress, in extending its membership ; and proposes certain modifications of the Constitution and By-laws, whereby he hopes that such defects will be overcome. As this subject concerns only the members, comment here would be out of place, although it may well be questioned whether any medical association could ever become popular with the profession, hedged in by the restrictions which Dr. Sims describes. To those who know him it is needless to say that his suggestions are urged with all the point and appositeness of illustration which characterize their distinguished author.

Of the papers that follow, the first is by Dr. Robert Battey, on the "proper field" for the special procedure of oöphorectomy devised by him, and which has rendered his name famous. As the points are not accurately formulated, although the paper is readable and interesting, no synopsis of it will be offered, but the reader referred to the original.

Two papers, which call for no special comment, follow—one by Dr. Engelmann, of St. Louis, upon Battey's operation in cases of anterior displacement of the ovary ; the second on the "successful extirpation of an encephaloid kidney." Nephrectomy is, however, in this country so rare in comparison with European experience—it is said to have been performed only three times in this city—that every well-marked case is worthy of careful record.

Next in order is a paper by Dr. A. Reeves Jackson, of Chicago, on "Uterine massage," in which minute directions are given as to the application of this method for the relief of chronic subinvolution of the womb, or hyperplasia, or uterine hypertrophy. The frequent association of perimetritis with this condition lessens greatly the class of cases in which such procedure would be employed profitably. But when we regard the extreme inefficacy of our ordinary resources in combating this phase of ill health, Dr. Jackson's suggestions, which are supported by clinical evidence, should receive the most careful consideration.

Clinical papers follow upon "Cataleptic convulsions cured by trachelorraphy," and "Ovariotomy during pregnancy." The first of these papers narrates an extremely well-observed case by Dr. Sutton, of Pittsburg, and demonstrates the remarkable results that may sometimes be obtained by Emmet's operation for laceration of the cervix uteri. The second, by Dr. Wilson, of Baltimore, led to a curious diversity of comment in the discussion that followed—some of the members advocating non-interference in such cases, others ovariotomy as soon as pressure on the uterus begins, and

others abortion in place of ovariotomy,—this last suggestion having certainly the merit of originality. No conclusion was reached, although to most readers the sensible one seems obvious enough.

These papers are followed by one from Prof. Parvin, of Indianapolis, viz., "Secondary puerpural metrorrhagia," which, without invidious comparison, may safely be termed the most complete and scholarly essay in the volume. It is practical, accurate in its statements, and scientific in its methods, and could only have been written by a man who was at once a student and a keen clinical observer. To be appreciated it must be carefully read and studied.

Three fatal cases of rupture of the uterus with laparotomy are next narrated in detail by Dr. Howard, of Baltimore, who proposes ablation of the uterus by a modification of Porro's operation as a substitute for the methods now in vogue for treating this appalling casualty. The proposition is bold but not unreasonable.

"Occlusion of the gravid uterus," by Dr. Eve, of Georgia, and a long and extremely elaborate dissertation on "Posture in labor" among different nations, by Dr. Engelmann, of St. Louis, follow next in order. The latter of these is profusely illustrated, and contains a mass of more or less useful information, from which the author deduces the conclusion that in ordinary labor cases, the semi-recumbent position of the patient should be adopted.

After this comes a short but practical and intelligently written paper by Dr. Chadwick, of Boston, on the "Hot rectal douche," which he recommends for two distinct classes of cases—-inflammatory conditions of the rectum and colon, and intrapelvic inflammations when accompanied by painful defecation and burning abdominal pain. Many clinical cases are given in corroboration of its beneficial effect.

This is succeeded by a dissertation on "Quinine in gynecic and obstetric practice," by Prof. Campbell, of Georgia, whose experience entitles his opinions to great weight. Like all who live and practise in highly malarious climates, he uses this drug more freely than in the North, and relies almost exclusively upon it.

Essays on "Manual dilatation of the os uteri," for the induction of premature labor, and upon the comparative value of "Laparotomy and laparo-hysterotomy for the removal of uterine fibroids," conclude the list of papers. The former, by Dr. Richardson, of Boston, is a plain and practical exposition of a method

well recognized by obstetricians as more rapid and effectual than the use of Barnes' dilators, etc., but in rash or unskillful hands fraught with the utmost danger. The second, by Prof. Palmer, of Cincinnati, is a careful and comprehensive study of hysterotomy, which will well repay perusal, and so complete that it must long remain of value to students or future writers on this subject.

The indices which conclude the volume are all exceedingly full and interesting; while that of "Gynecological and Obstetric Literature for 1879," is a monument of thoroughness and research. It reflects the utmost credit upon its authors, and would alone be worth much more than the cost of the entire volume to all students of gynecology.

[C. C. L.]

**Eczema and its Management.** A Practical Treatise Based on the Study of Two Thousand Five Hundred Cases of the Disease. By L. DUNCAN BULKLEY, A.M., M.D., etc. New York: G. P. Putnam's Sons, 1881, pp. 344.

Eczema, comprising, as it does, fully one third part of all diseases coming under the observation of the dermatologist, and presenting itself under so many guises, has, naturally, claimed a large share of the attention of those who write about skin affections. Necessarily, all systematic treatises upon dermatology discuss eczema at length, and indeed, so important has the subject seemed, that several bulky volumes have been devoted to it alone. Thus, in recent years, we have had works on eczema by Erasmus Wilson, McCall Anderson, and the late Tilbury Fox, and now we have this latest contribution from Dr. Bulkley, which finds, in America at least, an almost unoccupied field; for whatever may have been the cause, the English works just mentioned have failed to receive the favor in this country that one might have expected.

In estimating the merits of the work before us, it will be properly considered as divided into two parts. The first part treats of general questions relating to eczema. There is a chapter devoted to general considerations, the definition of eczema and its nosology, and one to general statistics. Chapter iii discusses general symptomatology and pathological anatomy; chapter iv, forms of eczema; chapter v, its diagnosis and prognosis; chapter vi, the nature of eczema; chapter vii, its etiology; and chapter viii, the treatment of the affection. Of the remaining eight chapters, six are addressed to the management of infantile eczema, of eczema of the face and scalp, the hands and arms, the feet and

legs, of the arms and genital regions, of the trunk, and, finally, of the general surface. The book concludes with a chapter on the diet and hygiene of eczema, and one on therapeutics.

In the consideration of a subject so many-sided as eczema, an arrangement such as the above, although inconvenient, appears to be unavoidable, and calls for a larger number of pages than one might deem necessary or desirable ; and indeed, we think the author *has* been unmindful of the advantage of saying what one has to say as concisely and in as few words as possible.

In the first eight chapters the author has had abundant opportunity to express his views upon eczema generally, and more especially its pathological and etiological relations ; views which, without doubt, will not be subscribed to by many dermatologists. Eczema is stated to be "a non-contagious, inflammatory disease of the skin, of constitutional origin, acute or chronic in character, manifesting any or all the results of inflammation at once or in succession, and accompanied by burning and itching." Notwithstanding his belief in the constitutional origin of eczema, Dr. Bulkley proceeds to confirm the accuracy of those who claim eczema as a catarrhal disease of the skin, despite the fact that catarrhs of purely local origin are of daily occurrence.

In seeking to establish his theory of the constitutional origin of eczema, we find the author ruthlessly discarding from the class of eczematous affections, diseases whose local origin seems to be assured. In this way, some forms of eruption that in all physical characters are eczematous are pronounced to be *not so*. The eruptions upon the scalp and body produced by pediculi are claimed as always being "dermatitis." And yet it is difficult to believe that many of the eruptions in the lower occipital region accompanying pediculosis capillitii, are any thing but eczemas. Eruptions produced by heat and cold, animal poisons, various vegetable substances, such as croton oil, savin, poison ivy, and arnica, aniline dyes, etc., he says, are never eczematous, although they may become the starting-points of true eczema, which may then remain and relapse again and again without recognizable local cause. Surely one frequently sees eczema, pure and simple, arising from one or other of the above-mentioned causes ; and indeed, at another page the author, obliged to admit this, asserts that it can only occur in those who possess the eczematous predisposition. To this we will refer in another place. That eczema, thus produced, tends to recover with the removal of the exciting cause, is simply because the tendency of the parts, less the irrita-

tion, is toward health. An irritant from without may be readily and entirely removed, while one residing in the part itself must offer more resistance and produce a more prolonged disturbance of nutrition.

A very valuable statistical summary gives the author's experience upon many features of eczema. His observations of the comparative frequency of eczema correspond closely with the much more extensive figures of the Statistical Committee of the American Dermatological Association; eczema affording  $34\frac{1}{4}$  per cent. of all skin diseases treated by him. Tables are given showing the ages of the patients, the duration of the complaint, the location of the eruption, the hereditary tendencies of the patients (respecting eczema). The author, for the most part, denies heredity in eczema. One table shows the frequency of the co-existence of eczema with other skin diseases. Of general diseases causing or accompanying eczema, the author found that malaria was almost without influence in this direction, while asthma affected eczematous patients so frequently, that he has come to consider "asthma, in many instances, simply a condition of the pulmonary mucous tract, similar to that found in the skin in eczema." It may be noted, as not a little strange, that while gout was detected with great frequency in eczematous patients, disease of the kidney, that so frequently results from the gouty state, was only rarely discovered; a statement that suggests the suspicion that the author's notion of the prevalence of gout is perhaps a little extravagant.

Chapter iii contains a description of the symptoms of eczema and of its pathological anatomy. This simply sums up the symptoms that may occur in the course of the disease, and no attempt is here made to give their clinical relations. Although Dr. Bulkley refuses to consider certain artificially produced eruptions as eczematous, he avails himself of the results of Neumann, from investigating a croton-oil eruption upon the ear of a rabbit, and, indeed, considers that the process does not differ from that of eczema. The pathological condition in chronic eczema, as studied by a number of histologists, is described, and attention is especially directed to the enormous distribution of nerve fibres to the skin, and to the probable influence of the nervous system in the pathogenesis of the disease.

No less than 125 Latin names have been collected by the author, that have been applied to the various phases of eczema. What wonder that so great obscurity has surrounded its study!

Order, however, is being gradually brought out of confusion. The author arranges the nomenclature in five classes of terms, to which is appended a sixth class of miscellaneous terms which will probably slowly pass out of use. These classes of terms relate: 1. To the stages of the eruption; 2. To the predominant lesion; 3. To the condition; 4. To the cause; 5. To the location. Thus, we speak of "eczema (1) chronicum, (2) papulosum, (3) fissum, (4) artificiale, (5) manuum." In the diagnosis of eczema, there are enumerated no less than 28 different affections liable to be mistaken for it. One of these is dermatitis (from heat, poison ivy, etc.), which, we are told, can sometimes only be distinguished from eczema by getting well quickly, and which may be the starting-point of a true eczema. Another affection, phthiriasis, may resemble pustular eczema very closely, "but the presence of pediculi and their nits is, however, sufficient for the diagnosis" (why not *call* it a pustular eczema from the irritation of lice?). The chapter on diagnosis is, however, well worked up, and we believe that about every point in diagnosis that can arise has been discussed. In the remarks on prognosis, eczema is pronounced to be a curable disease, but the author is wisely prudent in indicating circumstances interfering with recovery.

It is in his treatment of the question of the local or constitutional nature of eczema, that Dr. Bulkley will, undoubtedly, encounter much dissent. As we have seen, he unhesitatingly whisks over-board in preparing for action, various inflammations presenting the clinical features of eczema, as not eczematous. "If we accept one artificial eruption as eczema, we must accept all as such, from the large blisters following cantharides, heat, and cold, to the discrete pustular eruption produced by croton oil or tartar emetic, or the slightest erythematous blush caused by the mildest irritant." This is a remarkable statement. We usually recognize pathological processes by their general features and course, not from their exciting causes, and while we cannot recognize an eczema under all the conditions mentioned, we usually look upon an eruption presenting in every particular the features of eczema, as eczema; and fail to see any justification for any other conclusion. But, moreover, while we do not consider all these forms of eruption as eczematous, we do believe that they, in the main, depend upon processes essentially similar, from the sudden inflammation rapidly followed by free exudation, caused by a cantharidal plaster, to the slow chronic eczema proceeding from an irritation within the part itself. The difference lies in the intensity and transient char-

acter of the irritation upon the one side, and upon its mildness and persistence upon the other. In concluding that eczema cannot be a local disease at one time and a constitutional disease at another, the author surprises us, after the observations we have just quoted, in saying "that the eruptions resembling eczema, artificially produced, are either ordinary dermatitis with a strong tendency to spontaneous recovery, or are true eczema in eczematous subjects, in whom the exciting cause, instead of recurring in the ordinary way, has been artificially supplied, just as a gouty person might, by measures voluntarily applied, induce a true gouty inflammation of a joint."

The author proceeds to discuss the nature of eczema under seven headings. He first compares eczema with the acknowledged constitutional disorders of the skin, leprosy, contagious fevers, syphilis, etc. Here he finds resemblances in the symmetry of the lesions, etc. The conditions of the existence of these affections are so different, however, that comparison can shed no light on the pathology of either. Eczema does not depend upon a particulate principle, as we have every reason to believe the first-named affections do. Even if "the peripheral mode of spreading of eczema resembles much that of erysipelas," it can in this connection have no significance, since the processes are totally unlike. "Eczema is also not unfrequently attended with fever in its more acute and general forms." So is a cut finger. In contrasting certain local diseases of the skin with eczema, the former are claimed to be absolutely unsymmetrical. We would mention, as proving the contrary, two or three that just occur to us. These are acne, seborrhœa, hyperidrosis, some erythematous eruptions, even common warts, which are often decidedly symmetrical, or at least sufficiently so to equal in this respect many eczematous eruptions. From the study of microscopical anatomy, which is discussed at considerable length, absolutely no evidence favorable to the theory of the constitutional nature of eczema is to be found. Indeed, the author limits his inquiries to conjecturing whether the impulse to inflammation resides in the cells primarily and is only secondarily extended to the blood-vessels (cellular pathology), or whether the vascular disturbance is primary, and to what extent these processes depend upon nerve influence; questions of immense importance, it is true, but which have no bearing upon the point that the author seeks to establish, since in neither process is there involved, necessarily, a general blood alteration.

The next point considered is "the constitutional relations of

eczema as exhibited in its clinical history." It occurs at any age ; as frequently in the female as in the male. It relapses. We are told that eczema patients seldom pass large amounts of urine, the tendency being to scanty secretion, almost always unnaturally acid, with a specific gravity averaging above normal. It is unfortunate that Dr. Bulkley has not supported this assertion with a statistical table, such as he has so carefully prepared in relation to other features of eczema.

In discussing the theory of local pathology, Bulkley quotes from Hebra's latest edition, statements that go to show that this great dermatologist was willing to admit that there are certain conditions of the human organism that act as predisposing causes in the production of eczema. We insist, however, that the author utterly fails to grasp the true meaning of Hebra, and that there is a vast difference between disease of a part resulting from lowered constitutional conditions and disease of a part resulting from a definite constitutional disease. It would be absurd to deny that local diseases are largely under the influence of general conditions. Wherever we turn we find unmistakable evidence of this influence. Not to speak of those skin affections where the nervous system seems to supply the bond between them and diseases of various organs, every one must have seen skin affections improve with a general improvement of health, or relapse as the general health became impaired, or must have observed signs of cutaneous debility appear for the first time in those whose previous good health has begun to fail. But there is every reason to believe that in these affections simple imperfect nutrition and assimilation are sufficient to evoke the symptoms in a skin of deficient resisting power, just as they may evoke symptoms in other organs—the loci minoris resistentiæ—of the individual, without calling in, to become responsible for them, any specific blood alteration. That the skin of one person is more liable to suffer than that of another, is due to certain histological and nutritive imperfections in the skin of the particular individual, and we find similar deviations from health wherever we look for them. Many persons are more liable than others to disorders of the pulmonary mucous membrane, of the heart, of the kidneys, of the different portions of the alimentary canal, and this liability often remains throughout life ; and yet no one dreams of considering the morbid conditions thus arising, as dependent upon any specific morbid condition of the general system. (We are, of course, speaking of liabilities that have not been acquired through certain disease processes, as rheumatism,

for example). This conception of the influence of the constitution in the production of eczema, differs widely from that of Bulkley, who believes, "that as arthritic, pulmonary, or cerebral symptoms appear to be the culmination of blood processes which we know as gout and rheumatism, so eczema is directly dependent upon a somewhat similar, although as yet but little defined blood change," etc. At the end of this paragraph we find the inconsistent statement, "that the constitutional state may pass away spontaneously or under dietary, hygienic, and medicinal measures, while the *products* of the disease, the infiltration and consequent itching, remain." This, in the face of an earlier observation, that so long as the infiltration remains the eczema is not cured, but will pretty certainly return upon the suspension of treatment.

It is to be remarked that the author in directing constitutional treatment does not address his efforts to any peculiar eczematous systemic condition, but cures one case by giving diuretics and cathartics for renal disturbances, nitric acid for "biliaryness," remedies addressed to the nervous system, where that system is at fault, cod-liver oil to the scrofulous, iron to the anæmic. In a word, the disease is vanquished by restoring healthy nutrition to whatever portion of the organism has been at fault.

In considering the etiology of eczema, the author asserts that careful and repeated study and observation of patients with eczema will always show that they are not in perfect general health. The constitutional state "is one of debility, or lowered vitality of the whole system, or of one or more portions." We read that the pulse is rarely healthy, but may be variously abnormal ; but as it betrays no special characters, it can hardly be expected to throw much light on eczematous processes. Eczematous patients, we are told, may be divided into three tolerably distinct classes : those who are gouty, those who are scrofulous, and those who are neurotic. The recognition of these states is not always easy, but is important as bearing seriously upon questions of therapeutics. The gouty state as a cause of eczema receives extended notice. Then follows a long essay upon conditions of imperfect digestion, etc., which, though doubtless important and valuable, seems rather out of place in the present work. Undoubtedly many scrofulous children are eczematous, but then they likewise develop all sorts of other affections due to their scrofulous taint, displaying faulty nutrition in all parts. So, also, are many eczematous persons victims of nervous debility in various forms, as the author indicates; and it is of interest to observe that the symmetry so often ob-

served in eczematous eruptions he regards as evidence of the nervous relations of the disease, a supposition that is probably correct, and which deprives his theory of a blood disturbance of one of its props. As exciting internal causes of eczema are mentioned : indigestion, menstrual difficulties, lactation (producing a debility that results in the disease), dentition, varicose veins, and certain chronic diseases. It is difficult to believe that the author seriously considers the eczema so frequently developed in the legs of persons suffering with varicose veins of the lower extremity, as depending upon a special constitutional vice. Similarly, we suppose, he would attribute a like origin to varicose ulcers. Too much stress, he tells us, has been laid upon the importance of local causes in producing eczema. He considers their influence to be very slight, though he gives the usual list, with the reservation, that eczema results from such causes only in persons predisposed to the disease ; a manifestly true remark, which really means that the skin of some persons is more readily excited to eczematous inflammation than that of others, and implies, not the presence of a definite morbid condition of the blood, but a diminished power of resistance of a part.

Indeed, in considering the author's views concerning the pathology of eczema, we confess that we do not precisely grasp his meaning. He repudiates the older humoralistic doctrines, in claiming a constitutional blood origin to the disease. "This constitutional state is not represented by any one single definite condition ; there is no dyscrasia proper to eczema, as far as can be determined at present, although, for convenience, the term eczematous diathesis may perhaps best represent the totality of systemic conditions which are found in subjects of eczema." In support of this theory we are offered evidence that eczema occurs under a variety of conditions, in the scrofulous, in the gouty, in the neurotic, under circumstances of pronounced debility, or where the signs of imperfect health must be searched for with microscopic eye, but not a jot of proof that this hypothetical "diathesis," this specific blood condition, has a single sign of its own, except the cutaneous manifestations, which are far more reasonably accounted for by conditions of imperfect nutrition of the parts themselves, whether through faulty innervation, or whatever influence presides over their life.

It must be conceded, however, that his views of the pathology of the disease do not turn the author from sound principles of treatment. For the eczematous condition he has no specific. For

the acute stage we find recommended, mild laxatives and diuretics, and alkaline mixtures,—such treatment, in fact, as is employed in any mild inflammatory disorder. In chronic eczema the gouty, strumous, or neurotic state must be corrected. Much that he says of the constitutional management of eczema the author admits "pertains as well to other diseases and to persons exhibiting no eczema at all." The general health must be improved, the frequently existing constipation must be cured, the liver must be restored to healthy function. A valuable observation is the condemnation of the free use of enemata, as not meeting requirements, and the preference expressed for purgatives. Rational measures for the correction of digestive and urinary derangements are counselled. Arsenic is held in high estimation, though not for every case, and certainly not to the extent that is popularly believed in. It is strange that, despite the influence of nearly every modern dermatologist, arsenic continues to be the remedy persistently employed for every skin disease. To relieve itching gelsemium is especially recommended. The tincture should be used in ten drop doses, repeated every half hour until relief follows or until unpleasant physiological symptoms arise. If further experience confirm this action of gelsemium, it will prove a most valuable acquisition to our therapeutics. The directions for local treatment show the results of a large and carefully considered experience, and cannot fail to be of the greatest value to the profession.

It is with a sense of relief that we pass from general questions concerning eczema to the consideration of special forms of the disease and their management. Here we reap the harvest of the author's clinical experience, freed, to a great extent, from theoretical considerations. The chapter on infantile eczema has been very carefully prepared. The baseless fear of "driving in" an infantile eczema is exposed. Special attention is directed to the frequently associated formation of abscess, especially on the scalp and in debilitated children during the summer. These are manifestly due, though the author does not mention it, to the congestion of the sweat-glands, consequent upon their increased activity in warm weather, and are most often seen in the children of the poor in the alleys and crowded dwellings of large cities. While admitting that most eczematous children appear to be otherwise healthy, the author claims that a very rigid scrutiny will reveal some departures from a perfectly normal condition. And yet the deviation is, in many cases, very trivial. The highly detrimental

effect of the free, local use of water in these cases is insisted upon. Indeed, the subject is treated generally in a masterly manner.

Eczema of the face and scalp is described at considerable length. Of the author's skilful handling of this portion of his subject we have only praise. The directions for treatment are minute and comprehensive. Of all curable cutaneous affections, eczema of the hands is probably the most unamenable to treatment, and when cured, probably the most prone to relapse. This is readily explained by the liability of these parts to all sorts of irritation. The author's experience in the management of this form of eczema can but be valuable, but we fear that the hopes of the practitioner will be often disappointed, as heretofore, and unless a total change can be effected in the manner of life of the individual, his patient will often carry his eczema indefinitely. We think, however, that the therapeutic measures offered in this work promise as much relief as may be reasonably demanded. Eczema of the feet and legs deserves especial attention from the influence of position in its production. This is especially when the force of gravity, opposing the return of blood to the heart, is accompanied by dilatation of the veins. Eczema is a most common result of these passive congestions, especially when reduced general health further enfeebles the circulation. No plan of treatment that disregards this circulatory impediment will secure more than passing relief. The tissues must be supported, and to effect this, nothing equals the use of Martin's solid rubber bandage, first employed for this purpose by the author. The details for its application are given. It often affords incalculable relief. A method for the relief of the persistent and intolerable itching of eczema of the anus and genital regions, suggested by the author, is well worth remembering. The part should be enveloped in a handkerchief soaked in water as hot as can possibly be endured. This should be repeated at short intervals for several minutes, when, after careful drying, appropriate ointments should be applied. Cosmoline and vaseline, from which so much was expected as a basis for ointments, the author does not employ, on account of their not having body enough to remain as a thick coating on the cloth. They rapidly soak in, and leave the parts dry and exposed.

The final forms of eczema treated of are eczema of the trunk and general eczema. The eczema of the nipples and areola, described by Sir James Paget as often preceding cancer of the breast, receives due attention, as do also the various forms of general ec-

zema. Diet and hygiene of eczema are discussed in twenty-eight pages. This we consider inexcusable. We have already had occasion to object to the long disquisition upon gout and indigestion in an earlier part of the work. Not that we object to what the author says about these subjects. We willingly concede the accuracy of his remarks. But they might, with equal propriety, be applied to the discussion of any question of diet and hygiene, and bear no more upon eczema than upon a dozen other affections. In a work devoted to a special subject, one does not expect to have the number of pages needlessly multiplied by essays upon related, though not essential topics. The reader prefers to acquire his information upon these matters in the places where they properly belong, and to which the specialist would always do well to refer him, after having indicated their bearings upon his subject. The formulary contained in the concluding chapter will be found very valuable.

[I. E. A.]

**Lectures on the Pathological Anatomy of the Nervous System—Diseases of the Spinal Cord.** By J. M. CHARCOT. Translated by CORNELIUS G. COMEGYS, M.D. Cincinnati: Peter G. Thomson, 1881.

The subject-matter of the book of 160 pp. before us appeared in the *Progrès Médical* during 1879-80, and consists of lectures delivered at the École de Médecine by Charcot, reported by Dr. E. Brissaud.

To criticise a treatise on the spinal cord by Charcot would be an impertinence, and with regard to the work as a production of the author, it need only be said that it should be in the hands of every medical man not reading French. As a *résumé* of the known physiology, regional anatomy, and localization of systematic and other lesions of the spinal cord, particularly ascending and descending degenerations, it is, according to our present knowledge, perfect.

As a reproduction by translator and publisher it is open to criticism. The work of a translator, who puts at our command a valuable foreign production, is at best poorly paid and often thankless, and hence we should be led to temper our criticism with a grateful acknowledgment of our indebtedness.

The translation is good, with the exception of some very frequently recurring alliterations, as "inutile," "primitively," and "conservation," p. 64; "interested," p. 80; "biceps crural," p. 105; and such evident mistakes as "sensitive" for sensory, p. 49;

"nervous cell," p. 55; "locomotive ataxia," p. 128; and "contraction" for contracture, p. 131.

His use of "rotulian" for patellar is, of course, only a matter of taste. His word "reflexion" (our commonly accepted reflex) would look better, if he insists on a substantive, as reflection. The introduction of "lesed" as a past participle is, it seems, a rather disagreeable coinage and hardly a substitute for injured.

A word now to the publisher. In the last few years several translations, particularly two by Fowler, Charcot's Localization in Diseases of the Brain, and Benedict on Brains of Criminals, both published by William Wood & Co., New York, together with the present publication by Thomson, of Cincinnati, have thrust into the sensitive view of an American public wood-cuts vying in ugliness with the meat-axe productions of the daily press.

The cuts in the translations of Benedict are sickening parodies on the photo-lithographs of the German edition. It may be said of the cuts in the two translations of Charcot that they are fair reproductions of the originals, but that is not enough.

The style and finish of wood-cuts habitually seen in the *Progrès Médical* are not acceptable in a country which leads the world in wood-cutting. But further, when the primarily coarse stippling presents the appearance, in the reproduction, of having been made with a rock-drill (see Wood's Charcot, fig. 37, p. 123, and Thomson's Charcot, fig. 30, p. 79), and attempts at tint work in the originals are executed in the reproductions by an oyster-rake see (Wood's Charcot, fig. 21, p. 72, and Thomson's Charcot, fig. 15, p. 47), then comes the time for remonstrance against the parsimonious ways of certain publishing houses.

A wood-cut can be a perfect reproduction, as far as purposes of demonstration go, and at the same time be artistically far superior to the original, at a very little extra cost to the publisher.

[R. W. A.]

# ARCHIVES OF MEDICINE.

## Original Articles.

### NEUROSES OF SENSATION OF THE PHARYNX AND LARYNX, OR SENSORY NEUROSES OF THE THROAT.\*

BY LOUIS ELSBERG, M.D.,

NEW YORK,

PROFESSOR OF LARYNGOLOGY AND DISEASES OF THE THROAT IN DARTMOUTH MEDICAL COLLEGE.

#### SECTION III.—CLASSIFICATION.<sup>1</sup>

THE various sensory neuroses of the throat have not hitherto been strictly differentiated. This has been mainly because the three different kinds of sensibility of the throat have not been clearly appreciated. Although they are frequently affected together, each kind may be diminished, increased, or perverted separately; and an exaltation of one kind may be combined with a diminution or perversion of another. The correct position, in the classification, of neuralgia seems to have presented difficulties to observers, and systematic authors on the subject, such as Ziemssen, Mackenzie, Jurasz, etc., have described it as a fourth disorder of sensibility, separate and distinct from anæsthesia, hyperæsthesia, and paræsthesia.

Comprising disordered sensibility under the name dysæsthesia, my classification, based on clinical observations, not formed only "from theoretical considerations," is as follows:

\* By special appointment prepared for, and partially read before, the International Congress in London.

<sup>1</sup> Continued from these ARCHIVES, February number, p. 57.

<b>A. In reference to quantity or degree of sensation.</b>	<table border="0" style="width: 100%;"> <tr> <td style="width: 30%; vertical-align: top;"> <b>I. Diminished sensibility:</b>            Hypoesthesia.         </td><td style="width: 10%; vertical-align: top; text-align: center;"> <b>2.</b> </td><td style="width: 60%; vertical-align: top;">           1. Anæsthesia.            2. Analgesia (Analgesia, Anodynia).            3. Anesthesia dolorosa.            4. Tactile hypæsthesia and hypalgia.            5. Reflex hypæsthesia.         </td></tr> </table>		<b>I. Diminished sensibility:</b> Hypoesthesia.	<b>2.</b>	1. Anæsthesia. 2. Analgesia (Analgesia, Anodynia). 3. Anesthesia dolorosa. 4. Tactile hypæsthesia and hypalgia. 5. Reflex hypæsthesia.
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<b>DISORDERED SENSIBILITY : DYSÆSTHESIA.</b>		Peripheral. In nerve trunk or neuralgia. Cough, choking, spasms, retching, gagging, etc.  Muscular effects.  Circulatory effects. Flushing, paling.  Secretory effects.			
<b>B. In reference to quality or kind of sensation :</b>		<b>III. Perverted sensibility :</b> Parasthesia.			

The perversions of sensibility are so various that it is impossible to classify them or to enumerate them all. There are, however, perversions of tactile, of dolorous, and of reflex sensibility. Among the first mentioned, the most frequent are the sensation of heat or burning (the opposite—a sense of coldness—is very rare, but I have also seen that) and the sensation of the touch of a foreign body, attention being given mainly to its shape and its weight or pressure. Patients describe all possible and impossible shapes and materials in endeavoring to give an account of the perverted sensations. I have sometimes thought most of them might be arranged under the three heads: 1. Sphæresthesia, 2. Zonaesthesia, or strangleæsthesia, and 3. Alkanæsthesia ; comprising under the first, the sensation of a bulky body, generally ball-shaped ; under the second, a cincture or band feeling, a sense of constriction ; and under the third, the sensation of a sharp-pointed body, piercing or cutting. I shall give a few further particulars when speaking of symptoms.

## SECTION IV.—OCCURRENCE.

## α.—FREQUENCY.

"Anæsthesia, hyperæsthesia, paræsthesia, and neuralgia of the larynx undoubtedly may exist to a more or less well-marked extent, but whether they occur as separate and distinct affections, and are entitled to consideration as such, is certainly an open question. \* \* \* That paralysis, due to any morbid condition of the superior laryngeal nerve, such as neuritis, pressure of tumors, diphtheria, etc., will produce loss of sensation in the parts to which the nerve is distributed is undoubtedly true; but so long as this disease is described as one which might possibly occur rather than one which has been clinically observed, it would seem that its introduction into a treatise on these affections would be something of a refinement in classification." Such is the language of the most recent "Manual of Diseases of the Throat and Nose."<sup>1</sup> The fact is that sensory neuroses of the throat, though relatively infrequent, are not of very rare occurrence. Excluding the cases not coming fully within my definition and the explanation given, I have had under observation *at least* 58, which I have classified as follows: *Hypæsthesia*, 10, viz., complete one-sided anæsthesia, with hypæsthesia of the other side, 1; analgesia, 2; anæsthesia dolorosa, 1; tactile and reflex hypæsthesia, 3; reflex, without tactile or dolorous, hypæsthesia, 1; hypæsthesia with localized hypersensitiveness to temperature, 1; tactile and reflex hypæsthesia with some hyperalgesia and paræsthesia, 1. *Hyperæsthesia*, 27, viz., tactile and reflex hyperæsthesia, 10; tactile without reflex hyperæsthesia, 1;

<sup>1</sup> By Francke Huntington Bosworth, A.M., M.D., New York, William Wood & Co., 1881, p. 328.

In the preface the author says the book embodies the results of an experience of nearly ten years and of over eight thousand cases. According to my personal experience, in such a number there ought to have occurred a score of unquestionable instances of sensory neurosis. Perhaps I must not omit to state that Dr. Bosworth's cases were mainly those of a hospital out-door clinic.

tactile and reflex hyperæsthesia, with hyperalgesia from only external impressions, 2; tactile and reflex hyperæsthesia, with algesia both spontaneous and from external impressions, 2; neuralgia, 9; reflex, with very little or no tactile and dolorous hyperæsthesia, 3. *Paræsthesia*, 21.

As to sex and age, there were 26 male and 32 female patients, of whom the three youngest were a girl of seven and a half and two boys of eight and nine years respectively, and the two oldest a woman of 58 and a man of 65 years. Altogether there were

Under 10 years, 3 cases, viz., 2 male and 1 female.						
Between 10 and 20	"	7	"	2	"	5
" 20	" 30	" 13	"	5	"	8
" 30	" 40	" 19	"	8	"	11
" 40	" 50	" 11	"	5	"	6
" 50	" 60	" 3	"	2	"	1
Over 60						
	"	2	"	2	"	

The greatest number of both sexes were between 30 and 40 years old. The rise and fall as to age in the number of male patients is remarkably uniform. The number of females exceeds that of males in every decade except in those under 10 and over 50; the total excess is not more than 23 per cent., which is much less than is usually supposed to be the case in such neuroses.

#### b.—DISEASES OR PATHOLOGICAL CONDITIONS IN CONNECTION WITH WHICH SENSORY NEUROSES OF THE THROAT HAVE BEEN OBSERVED.

I. *Paralysis*.—Although the term paralysis embraces impairment of sensation as well as of motion, yet when used without qualification, only deranged muscular activity, either neuropathic or myopathic, is usually meant. Throat dysæsthesiæ, *i. e.*, anaesthesia, hyperæsthesia, and, to some extent, paræsthesia, occur in many cases of paralysis; they have been observed in cases of laryngeal paralysis from

disease or injury of the pneumogastric nerve. I have met with them very early in a case of progressive paralytic dementia, *i. e.*, paralysis of the insane, in the course or later stages of which disease they are known to every alienist. Krishaber has reported them in early, and Ziemssen in advanced, progressive bulbar paralysis, *i. e.*, Duchenne's progressive paralysis of the tongue, velum, palate, and lips, of which I have seen two cases, one of which I have watched from beginning to end.

I have had an unmistakable case of paralysis of the superior laryngeal nerve, which I have not included in the number of cases mentioned in this paper, because the notes of it—though a hospital case—are unfortunately mislaid. I have had an opportunity to observe a case of paralysis of central origin involving one side of the body and deglutition, breathing, and phonation as well as sensation of the throat.

In a case in which another surgeon had performed œsophagotomy for the successful removal of a set of artificial teeth, I found hypæsthesia and paræsthesia combined with paralysis of the inferior laryngeal nerve. Of the paralysis of hysteria, diphtheria, syphilis, etc., occurring in combination with these sensory neuroses, I need not here speak as I am about to take up these diseases separately.

Dr. A. H. Smith has made the ingenious suggestion that in some cases of paræsthesia "the phenomena might be explained by assuming that there was a slight paresis of some of the muscles of the throat, leaving others without sufficient antagonism."

2. *Hysteria, hypochondriasis, and neurasthenia.*—These morbid conditions, more than any other, are regarded as being connected with the dysæsthesiæ under consideration. Certainly nervous exhaustion, from excessive intellectual exertion or emotional or sexual excesses, masturbation,

etc.; the curious nervous condition called hypochondriasis or sometimes pathophobia or psychical hyperæsthesia; and the still more mysterious hydra-headed disorder called hysteria, give rise, directly or indirectly, to the most strange sensory neuroses; nevertheless a large proportion of the sufferers from throat dysæsthesiæ are not only not at all hysterical but also not in any way "nervous." According to Chairou, anæsthesia of the epiglottis and of the pharynx is so constantly present in hysteria that he regarded it as a pathognomonic sign. Sawyer also insisted on the frequency of its occurrence. On the other hand, Mackenzie said he had observed slightly diminished sensibility of the pharynx, but never that the mucous membrane of the larynx was at all obtuse to direct impressions; whilst Semon, Mackenzie's German editor, acknowledges that he has seen it in several cases, though not in every case.

The truth is, anæsthesia is by no means the most frequent disordered throat sensation which is met with in hysteria. Thaon found it in about one sixth of the cases. Hyperæsthesia, as well as paræsthesia, are far more frequent. To the latter belongs the well known *globus hystericus*.

3. *Chlorosis and anæmia*.—Closely related to the cases of nervous exhaustion in which sensory neuroses of the throat occur, are those of chlorosis and anæmia; the former especially connected with menstrual troubles; the latter, in both sexes, after great loss of blood, after severe illness, etc. The forms usually met with are those of hypæsthesia or else neuralgia, but occasionally also tactile and reflex hyperæsthesia and paræsthesia.

4. *Diphtheria, syphilis, and malaria*.—The fact that more or less complete anæsthesia of the throat may follow in the wake of diphtheria has long been known. Careful and accurate observations as to this condition, were, however, first made by Ziemssen, afterward by Schnitzler.

Paræsthesia may accompany hypæsthesia. As to syphilis, Ott has published a detailed account of an interesting case.

I have had under my care a case of localized painful hypæsthesia—anaesthesia dolorosa—and another of neuralgia, in both of which no other cause could be recognized than malaria.

5. *Drug poisoning.*—Aside from general anæsthetics and narcotics, there are drugs which affect the sensibility of the throat. The paræsthesia produced by some, as for instance, the sensation of dryness by belladonna, of tingling by aconite, of constriction by nux vomica, etc., are well known. Bromides produce hypæsthesia of the throat, and in a case of saturnine aphonia which I have treated, there was said to be anæsthesia of the throat, which, unfortunately, however, I did not sufficiently carefully examine to record.

6. *Genito-urinary, pulmonary, and other diseases.*—Disordered sensations of the throat sometimes depend upon disease in other portions of the body. Cases in which this occurs are of two kinds: those in which a special nervous connection between the organ affected and the throat is known to exist, and those in which such a connection cannot be traced. A common instance of the first kind is a throat dysæsthesia from ear trouble. A still more frequently observed instance of the second is a throat dysæsthesia from genito-urinary, particularly uterine, disease. The first is generally supposed to be easily explicable as a "reflex," starting from the auricular branches and ending with the sensitive laryngeal branches of the pneumogastric nerve; while the second is admitted to be a recondite sympathy. But the first is not always easy of explanation: for, a reflex action such as this would be opposed to received physiological principles. As Hart has shown,<sup>1</sup> for a reflex action

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<sup>1</sup> *Practitioner*, London, July, 1878, p. 342.

afferent and efferent fibres are necessary ; the former are of necessity sensory ; the latter may be motor, vaso-motor, vaso-inhibitory, cardio-inhibitory, or secretory. They are never sensory, for the simple reason that a sensory nerve is always afferent ; and Hart suggests, in place of the reflex theory, the hypothesis of extension of irritation from one nerve centre to an adjacent one. Such phenomena may also be due, as Woakes suggests,<sup>1</sup> to "vascular distension of the sensitive tissues in the region where the pain is appreciated, brought about by implication of vaso-motor nerves."

I agree with Woakes that "though some links in the chain may here and there be missing,—and some lines of impulse may be wrongly traced,—yet when a larger knowledge of the anatomy and physiology of the vaso-motor system is attained, allowing these errors to be rectified, the theory here broached will hold its ground and prove of much wider applicability."

In other cases, where there is exalted impressionability of the nervous apparatus of the throat, the explanation of a dysæsthesia may be that suggested by Arndt<sup>2</sup> for so-called "co-sensations" or paradoxical sensations. He says : It may happen that a certain excitant which acts upon a particular nerve produces not alone a sensation in the part of the sensorium to which it passes, but that the impressions of this part, on account of the exalted sensibility of another part more or less connected with it, are taken up by this other part. In such cases there are produced, aside from the normal sensation, others, *i.e.*, abnormal ones. Thus, for instance, a slight pressure of the boot produces not alone sensation of pressure in the foot, but headache, etc., etc.

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<sup>1</sup> "On Deafness, Giddiness, and Noises in the Head," by Edward Woakes, M.D., London, 1880, 2d ed., p. 79.

<sup>2</sup> Eulenburg's "Real Encyclopædie der Gesammten Heilkunde," vol iv, p. 532.

Disordered sensations of the throat also occur in pulmonary affections, especially during the course of pulmonary phthisis, and even very early; being more distinct on or limited to the side of the affected lung, and under circumstances when organic laryngeal lesion must be excluded. Jurasz has recorded a case in which they formed a complication of croupous pneumonia.

I have stated that I exclude from present consideration the diminished sensibility during epileptic paroxysms, but sometimes this hypæsthesia persists, and, as in Spaack's case, is observed in the interval.

Rheumatism and gout have been accused of leading to sensory throat neuroses.

When foreign bodies have been lodged in the throat, though removed and though no traumatic effects have been produced, they often leave the sensation of their presence for a long time, and sometimes the mere belief that a foreign body has entered the throat makes the person feel similar symptoms.

Occasionally catarrhal and other organic throat affections leave disordered sensations behind;—and here it must be emphasized that whenever such local disease is found to co-exist with them, they are out of all proportion, sometimes even in no relation therewith, persist after the local lesion is cured, or sometimes are cured while the lesion remains.

Throat dysæsthesiæ dependent on various diseases of the body are discussed in my essay, published many years ago, on "The connection of throat and other diseases."<sup>1</sup>

#### SECTION V.—CAUSES.

In the list of the morbid affections leading to sensory neuroses of the throat I have given a number of causes which, to avoid repetition, I shall not mention now. Pre-

<sup>1</sup> New York, 1870, a reprint from *Medical Gazette*, Jan. 22, 1870.

viously I had stated that, excluding the structural throat lesions which themselves involve the peripheral sensitive nerves and the brain lesions of insanity, there must be affected, in the neuroses under consideration, the respective nerve tracts or their nuclei; that such affection may come from traumatism, from pressure by tumor, foreign body, etc., from circulatory perturbation, or from other disease or injury, I need not dwell upon. Certainly the great ordinary *predisposing* cause of these neuroses is that general nervousness of constitution which is really "the child of civilization and mental culture, of refinement in clothing, food, and dwelling-place, of want of proper physical and muscular exercise," and which has been perpetuated by hereditary influences. Patients may belong to this class of nervous individuals without being at all hysterical or hypochondriacal, or what is commonly called "nervous"; yet they are more than ordinarily impressionable, are of so-called "neuropathic disposition." Of the *exciting* causes, the most frequently assigned by patients themselves is "taking cold"; and though that phrase is sometimes only ignorance-cloaking and meaningless, nevertheless exposure to atmospheric change, draft, the extremes of heat and cold, especially sudden change from one to the other, combined with wet,—is often, in persons predisposed, the only etiological starting-point which the most careful scrutiny can detect. Sometimes this is true of inhaling smoke or dust, or of taking at particular times alcoholic stimulants, or wine, or even coffee, in cases in which not the slightest local effect on the mucous membrane can be detected. Intense intellectual labor or emotional or other excesses, which usually can be regarded as predisposing causes, also occasionally act as exciting causes. I have had a case in which great sexual excitation in a perfectly healthy unmarried female was followed immediately by intense pain in the

throat, which required months of treatment before it could be relieved. Since she has been married, every sexual intercourse causes a temporary return of the throat pain.

When speaking of the fact that foreign bodies, after removal, sometimes leave for a long time disordered sensations behind, I referred to the curious circumstance that sometimes the belief of the presence of a foreign body causes the same subjective symptoms. It almost seems that in nearly every case psychical, either as predisposing or exciting, co-act with other causes in producing a throat dysæsthesia. Strong emotions always increase existing dysæsthesiæ, and I have had abundant proof that they alone, under many circumstances, are sufficient for their production. In families in which one or more members have suffered from severe, especially chronic, throat disease, either constitutional or local, I have several times found a sensory neurosis of the throat affect persons who were and remained absolutely free from structural lesion. In such cases, fear alone seems to be the efficient cause. Just as motor disturbances are recognized, that are dependent on an idea, on emotional attention directed strongly upon a particular part of the body, so such sensory derangements occur, entirely independently of hysteria, hypochondriasis, simulation, or even imagination, so far as this latter word carries with it the idea of unreality. Continued examination of the throat sometimes makes it temporarily oversensitive; and in some cases hyperæsthesia is observed without any cause, as an idiosyncrasy.

#### SECTION VI.—SYMPTOMS AND DIAGNOSIS.

The symptoms are mainly subjective, but in both hypæsthesia and hyperæsthesia, there are also objective symptoms, especially when the reflex sensibility is affected.

i. *Hypæsthesia*.—Diminution of sensibility may vary in

different cases from a slight bluntness of feeling to a complete absence ; and it is only in the latter case, *i. e.*, in that of loss of sensation, that the term anaesthesia should be used. The diminution may be more or less circumscribed or extensive, unilateral or bilateral, or, if on both sides, greater on one side than on the other. Instead of anaesthesia it is much more common to find a diminution or absence of reaction to impressions that normally should cause pain, *i. e.*, hypalgia or analgesia,—while at the same time the sensation of contact of a body and the reflex reactions of cough, gagging, etc., are only very little impaired. Sometimes pain is felt, spontaneously and in response to external impressions, at the same time that both tactile and reflex sensibility are diminished or lost, a condition which is called anaesthesia dolorosa. Sometimes the reflex re-action is very feeble, while contact and pain are normally felt; while at other times the reflex sensibility is intact or even exalted, and all other sensitiveness lessened. In one case I noticed, in a very intelligent and otherwise healthy female patient of about 34 years, the curious phenomenon of delay of appreciation, *i. e.*, several seconds elapsed before reaction occurred.

As a constant symptom, objective always, and frequently also subjective, *i. e.*, when the patient becomes conscious of it, I call attention to the accumulation of saliva and phlegm in the throat. This is sometimes in the valleculæ, sometimes in the pyriform sinuses, or in both. I do not remember to have seen a well-marked case of hypæsthesia in which this symptom was not present to some extent ; and I call special attention to it because, when the patient is not aware of it, and the accumulation is not very abundant, it is apt to be overlooked. Hypæsthesia is accompanied with difficulty of swallowing and “wrong swallowing,” or food passing into the wind-pipe ; but this is due less to the in-

sensible condition of the parts than to associated muscular paralysis.

2. *Hyperæsthesia*.—The term hyperæsthesia is frequently used as though it were synonymous with hyperalgia, *i. e.*, increased painfulness. Bristowe,<sup>1</sup> while recognizing the proper meaning of the term, justifies this use because, as he says, “practically exalted sensibility is scarcely, if ever, distinct from painful sensibility.” As the rule, this is unquestionably true, although each of the three kinds of sensibility is liable to be exalted alone, or together with one or both of the others. Hyperæsthesia may vary in different cases in degree and extent, in the same manner as hypæsthesia.

When the tactile sensitiveness alone or together with the dolorous is increased, the name “*oxyæsthesia*,” meaning simply very sharp or acute sensibility, has sometimes been given to it. I have had a case of a gentleman who could distinguish—and painfully so—the points of my æsthesiometer two millimetres apart in almost every portion of his throat. Hyperæsthesia is sometimes so great, that not only contact but even approximation of a body is sufficient to produce reaction. This ideal reaction—especially reflex—occurs in cases of idiosyncrasy more often than in acquired hyperæsthesia.

When the dolorous sensibility is affected the pain is felt sometimes only when the parts are moved, as during swallowing, speaking, etc.; sometimes spontaneously as well. I have had several cases in which the pain more or less completely interfered with the use of the voice, producing what Coën, of Vienna, first described as phonophobia: in one case, even the slightest whisper caused agonizing pain. Spontaneous pain is sometimes continuous but more usually intermittent or at least remittent. When periodic, it frequently returns at the same

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<sup>1</sup> “Theory and Practice of Medicine,” London, 1876, p. 932.

time of the day. It is always made worse by strong or sudden emotions, and sometimes disappears and reappears without assignable cause. True neuralgia unquestionably occurs, although the diagnosis in some of the cases reported as such may be questioned. The pains, usually in the front part of the throat and neck, are paroxysmal and flash with momentary intensity along the course of the nerves. Sometimes they are also felt in the back of the throat and in the neighborhood of the tonsils, and sometimes, though more rarely, extended peripherally as well as along the course of nerve fibres.

Occasionally the pains radiate from the throat as a centre to the neck, back, head, shoulders, arms, etc. The symptoms of reflex hyperæsthesia as ordinarily seen consist mainly of muscular contractions, such as retching, gagging, nausea (to vomiting), cough, expiratory spasm, inspiratory spasm, etc. I cannot here take these up in detail but would say that one or several of these may be present, while, at the same time, others and hyperæsthesia of another kind of sensibility, are absent. This is frequently the case in so-called "nervous laryngeal cough," which is a particular phenomenon of reflex hyperæsthesia. The circulatory and secretory effects are to some extent more recondite symptoms of reflex hyperæsthesia. The occurrence of lachrymal and nasal hypersecretion, as well as abundant flow of saliva and phlegm in response to impressions limited to the throat, is of course well known, but the reaction does not end here. No laryngoscopist can have failed to have noticed that perfectly healthy vocal bands are sometimes suddenly temporarily flushed and as suddenly paled, or that the mucous follicles in some part of the throat under his eye momentarily secrete from over-sensitiveness alone.

3. *Paræsthesia*.—Positive symptoms of paræsthesia are entirely subjective. Patients complain of perverted sensa-

tions, which may be referred to one or the other of the three kinds of sensibility, alone or in combination : there may be the spontaneous sensation of itching or scratching; of so-called formication or vermination ; of heat, *i.e.*, burning, or cold ; of dryness, of weight or pressure, of roughness, or rawness. Generally there is a sensation of the presence of some kind of a foreign body, either stationary or moving about, which may be a hair, a fish-bone, a burr, artificial teeth, a fly crawling about, or something else, which the patient believes has entered his throat. Sometimes there is a sensation of vacuity, of fulness, of stiffness, etc.

The three main classes of perverted feelings are, as I have already said, sphæraesthesia, zonæsthesia or strangalæsthesia, and akanthæsthesia, *i.e.*, of a ball- or globe-shaped body, of a cincture or halter, and of a sharp point. The first gives the feeling of fulness, the second of constriction, and the third of piercing. It is impossible to enumerate all the varieties of these classes. The symptoms exceptionally persist uninterruptedly day and night, interfering with sleep ; sometimes they are absent for an hour, or a day, or several days, and then return. There is objectively found in cases of paræsthesia occasionally more or less hypæsthesia, but far more frequently hyperæsthesia. In these cases all sorts of pains are described—stinging, cutting, boring, crushing, etc. I am inclined to mention here, as an instance of perverted sensation, the case of a young lady with chronic hypertrophic catarrh, who positively experienced pleasure from applications to her naso-pharynx, which ought to have (*i.e.*, in the vast majority of cases in my experience, decidedly have) given considerable pain. I have, however, counted neither this case nor a few more or less similar cases among the 58 enumerated in this paper.

*Diagnosis.*—From the symptoms and, in appropriate cases, the æsthesiometrical examination, a diagnosis can easily be

arrived at, provided it be certain that there is no structural change present. This latter point can of course not be ascertained without a thorough pharyngo-laryngoscopical examination. *While, on the one hand, great care must be taken not to overlook local lesions,—and special attention should be given to the comparatively less accessible portions of the throat,—on the other, the mistakes must be avoided to regard as such local lesions the reflex effects of disordered sensibility, and to give undue prominence to consequential, unessential, and unrelated local complications.* Errors of diagnosis in all these directions have come under my observation, and were I not forced to content myself in this paper with this passing mention, I should like to refer to the subjects of elongated uvula, the actual presence of a foreign body, or small out-of-the-way ulcer, etc.

Frequent "wrong swallowing"—the tendency of food to pass into the larynx—in the absence of any obstruction to the entrance of food into the stomach, justly makes us suspect hypæsthesia of the throat. Under these circumstances an accumulation of saliva and phlegm strengthens that suspicion; but æsthesiometrical examination alone proves it to be true. Whenever the throat is complained of, especially when swallowing and the use of the voice, either or both, are attended with pain or any unpleasant sensation, in the absence of a local lesion to account for the symptoms, we must suspect hyperæsthesia or paræsthesia. In all such cases æsthesiometrical examination must be instituted. I have already described the manner in which the three kinds of sensibility are examined by means of my æsthesiometer. In the absence of this instrument, laryngeal probes, blunt-pointed and sharp, will answer the purpose except for the differential determination of the sensitiveness to temperature. In determining whether the sensibility is diminished or increased at any point, if one-

sided, we must compare it with what is known as the healthy side, and always with other portions of the throat in relation to the order of acuteness of sensibility which I have presented as a physiological basis, remembering, however, that the list given is a provisional and imperfect one. Unless difference of sensitiveness in one direction or the other is very considerable, we must be cautious in trusting to it. I have pierced the velum with the sharp point of the aesthesiometer, so that it bled, without the patient's feeling more than a contact; and Jurasz has reported a case of diphtheritic anaesthesia in which puncturing the posterior palatine arch on the right side, causing bleeding, was not even felt at all. It is so easy to graduate the strength of the electrical current in most Faradaic apparatuses, that I avail myself, by means of throat electrodes, of this current for determining the point at which the patient experiences pain or any sensation at all,—comparing the strength with that which is appreciated in other portions of the throat or other mucous membranes. On applying the galvanic current, some sensitive points are sometimes found (distinct from Valleix's painful points in the course of nerves in neuralgia) to which Fraenkel has called attention.

The general condition of the patient, the diseases which complicate disordered sensations, and the peculiar character of the symptoms in disappearing and returning and being influenced by psychical circumstances, help to determine the neurotic element in the case.

#### SECTION VII.—PROGNOSIS.

The prognosis should in all cases be cautiously framed. Sometimes the disorder is very grave. Generally the duration is uncertain, but palliation possible, even if cure is unattainable. Frequently, treatment, though difficult, is successful.

The danger of hypæsthesia comes from "wrong swallowing." A bolus may suffocate the patient, or fatal so-called "food pneumonia" may result from the entrance of food into the air-passages. The danger in hyperæsthesia comes from difficulty of swallowing and from the effects of the irritation of the reflexes upon the general health. In paræsthesia the psychical effects have a tendency to undermine health.

#### SECTION VIII.—TREATMENT.

The patient's general health should, in all cases of disordered sensation, receive attention; and occasionally nothing more need be prescribed than change of air, travelling, a course of hydropathic treatment, or tonics and alteratives. The proper psychical treatment is also of importance. Always in hypæsthesia, and sometimes in hyperæsthesia and paræsthesia, quinine internally, and by insufflation locally, is especially useful. I have cured recent and slight cases by an emetic. Locally, I have found the frequent use of my "throat educator"—originally devised to overcome the hyperæsthesia interfering with laryngoscopical examinations, and consisting simply of a smooth piece of hard rubber or wood—of more or less benefit in nearly every case of sensory neurosis. Any other instrumental appliance, or, perhaps, the finger, might do as well, but it must not be forgotten that a great deal depends in these neurotic cases upon psychical impression. Sprays of pure and medicated water, either hot or alternately hot and cold, forced into the throat with a pressure of from five to twenty-five pounds; applications, by sponge and brush, of a saturated solution of iodoform in sulphuric ether; and, finally, electricity, induced and dynamic, have served me good purposes in the most varying cases of these disordered sensations.

In addition :

In hypæsthesia I have used internally, phosphorus, and nux vomica,—strychnine, also, by insufflation, and hypodermically (metallo-therapy and quite recently xylotherapy have been recommended); and, if, in swallowing, food enters the larynx, the patient must of course be fed by means of an œsophageal tube.

In hyperæsthesia we have, in the bromides, especially potassium bromide, both internally and locally, and in morphine, dissolved in mucilage, used locally, and hypodermically to produce its general anodyne effects,—agents which temporarily, and, sometimes, permanently, control the disordered sensations. Neuralgic pains are sometimes temporarily relieved by aconitine ointment, externally applied, sometimes by equal parts of camphor, chloral, and chloroform, externally, and sometimes even by ether spray externally. Internally, three-grain doses of mono-bromide of camphor every few hours, or fifteen minims of hydro-bromic acid in water every four hours, have been strongly recommended.

In paræsthesia, besides the general and special treatment already indicated, I have seen occasional good effects from zinc phosphide, zinc cyanide, belladonna, and ergot.

In conclusion I would say, that whenever there are present, in cases of dysæsthesia, any local lesions, they should of course be treated, and if possible cured. Their continuance does sometimes keep up the disordered sensation, although their removal is not always followed by its cessation.

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## RATTLESNAKE VIRUS: ITS RELATIONS TO ALCOHOL, AMMONIA, AND DIGITALIS.

By ISAAC OTT, M. D.

THE snake poisons have been the subject of but little investigation except by Drs. S. Weir Mitchell and Fayrer. This is somewhat surprising, considering the importance of the subject to every man travelling in country districts. I have mapped out a series of researches upon all the poisonous snakes procurable, with the view of studying the physiological action of their special poison and the means of counteracting its effects. In this paper I intend to confine myself to the action of the poison on the circulation and the agents which are powerful in sustaining it. The circulatory changes were studied by means of Ludwig's kymographion, so that during each second I could observe the counteracting effect of the drugs named. The snake was enclosed in a box which could be separated into two compartments by a sliding door. The snake was put into one apartment and the rabbit or his leg into the other, when the sliding partition was raised and the snake allowed to strike. Then the kymographion was attached as soon as possible, and the changes noted or the drug injected. I shall first give the general action of the poison of the rattlesnake upon the circulation.

*Exp. 1.—Rabbit struck on the foot by the snake.*

TIME.	PULSE.	PRESSURE.	
10.15. 0 A. M.,			struck by snake.
10.15.15 "	83	81	
10.15.30 "	90	81	
10.16.30 "	79	78	
10.17.30 "	75	50	
10.18.30 "	80	72	
10.21.30 "	81	68	
10.23.30 "	82	68	
10.36.30 "	82	72	
10.50.30 "	80	69	
11.20.30 "	death ; chest opened ; heart beating slightly.		

*Exp. 2.*—A little rabbit was struck by the snake, after he had rested four days, at 11.50 A. M. Defecation and urination ensued. Animal sits perfectly still; weak voluntary power over the extremities. 12.15 P. M., cardiac movements very feeble; posterior extremities spread outward; sinks down. 1 P. M., death; heart making a few trembling, weak movements.

*Exp. 3.*—Large rabbit was struck by a snake after a rest of a week. The pulse was too weak to be registered; chest opened; heart making trembling, imperfect systoles. Ammonia, when driven into the lungs, did not rouse the heart or increase the pressure. Death took place by convulsions in about a half hour after the snake struck the animal. Right auricle beat three hours after death.

*Exp. 4.*—Rabbit was struck by snake, after he had rested about four days, at 9.56 A. M. Was struck in nose and at root of ear. 10 A. M., tremors in both posterior extremities. 10.6 A. M., respiration very quick, panting. 10.8 A. M., registration of pulse began.

TIME.	PULSE.	PRESSURE.	
9.56. 0 A. M.,			snake struck animal.
10. 8.15 "	64	80	
10. 8.30 "	63	72	

TIME.	PULSE.	PRESSURE.
10. 9.15 A.M.,	58	60
10.10.30 "	59	68
10.11.45 "	58	66
10.13. 0 "	58	62
10.14.15 "	60	68
10.15.30 "	59	65
10.16.45 "	62	74
10.18. 0 "	62	72
10.19.15 "	66	70
10.19.30 "	62	68
10.20.30 "	64	70
10.21.30 "	65	68
10.22. 0 "	60	66
10.29. 0 "	animal died.	

The pulse and pressure, after the injection of the virus subcutaneously, gradually fall, especially the pressure, which decreases greatly. The fall of pulse is not very great till near death. In one experiment there was a momentary increase of pulse before the fall, whilst after the cessation of breathing, the heart beats in a trembling manner for a short time and then lies in a relaxed, flattened condition. Yet I believe that the main cause of the arrest of respiration is mainly due to the weakened systoles and low pressure. Occasionally when the heart had stopped electrical stimuli would not excite it. The independence of the respiratory centre is greatly interfered with by cardiac weakness, which I believe to be the main cause of death in the cases here noted.

That the snake may kill in a few minutes is quite true, and the cause here may be due to other coöperating influences in a large part. In the study of the antagonism of drugs to the rattlesnake poison, it must be remembered that rabbits are usually killed by it, whilst the dog, according to Dr. Mitchell, rarely dies, so that the drug must be quite effective to prevent death in the animals experi-

mented upon. It must also be noted that the snake had been used on an average every week or two for an experiment for four months. To study the antagonism of the above-named drugs they were injected through the jugular toward the heart, when the pulse and arterial tension were being registered. I shall give examples with liquor ammoniae.

*Exp. 5.*—Rabbit struck by a snake after he had rested ten days.

TIME.	PULSE.	PRESSURE.	
2.42. 0 P. M.,			snake struck animal
2.50. 0 "	60	34	
2.50.15 "	53	36	$\frac{1}{2}$ gtt. of ammonia in 1. 5 c. c. of water.
2.50.30 "	43	48	
2.50.45 "	56	54	
2.51. 0 "	58	50	
2.51.15 "	57	40	
2.51.30 "	60	52	
2.52. 0 "	65	56	
2.52.15 "	67	54	
2.52.30 "	71	46	
2.54.30 "	69	44	
2.54.30 "	70	40	
2.55.30 "	67	40	
2.57. 0 "	50	24	$\frac{1}{2}$ gtt. ammonia in 1. 5 c. c. of water.
2.57.15 "	53	34	
2.57.30 "	47	30	
2.57.45 "	60	24	
2.58.45 "	63	20	
3. 2.45 "	62	32	
3. 7.45 "	26	10	$\frac{1}{2}$ gtt. ammonia in 1. 5 c. c. of water.
3. 8. 0 "	24	4	
3. 9. 0 "			death.

This and other experiments demonstrated that ammonia

temporarily increases the pulse and pressure in rattlesnake poisoning, but that finally its injection in an almost exhausted heart rapidly stops it.

The next drug tried was alcohol.

*Exp. 6.*—Rabbit struck by snake after a rest of four days, just before ecdysis.

TIME.	PULSE.	PRESSURE.	
1.23. 0 P. M.,			snake struck.
1.35. 0 "	76	40	
1.36. 0 "	80	36	Five minims of alcohol in 16 cubic centimetres of water slowly introduced.
1.36. 5 "			
1.36.15 "	60	64	
1.36.20 "	death.		

*Exp. 7.*—Rabbit, snake rested nineteen days; ecdysis going on, panting respiration; struck at 8.30 A. M.; 9 P. M. death, although ten minims of diluted alcohol were injected through the jugular five minutes before death; chest opened, heart making trembling movements.

*Exp. 8.*—Rabbit struck by snake after he had rested fourteen days.

TIME.	PULSE.	PRESSURE.	
4.40. 0 P. M.,			snake struck.
4.49.45 "	64	42	10 minims of alcohol in eight c.c. of water.
4.50. 0 "			
4.51. 0 "	59	54	
4.52. 0 "	61	54	
4.53. 0 "	74	46	
4.54. 0 "	74	48	
4.55. 0 "	56	50	
4.55.15 "	38	34	
4.55.30 " animal dead.			

The experiments with alcohol show that it has the power to temporarily stay the falling arterial tension and the decrease of pulse.

I shall give an experiment with digitalis. The officinal infusion was used.

*Exp. 9.*—Large powerful rabbit was struck twice, once through the middle of the ear; snake had rested twenty-seven days.

TIME.	PULSE.	PRESSURE.	
12.27. 0 P. M.,			snake struck.
12.35. 0 "	79	82	
12.35.15 "	82	78	6 c. c. infus. digitalis.
12.35.30 "	72	83	
12.35.45 "	60	108	
12.36.15 "	59	116	
12.37.15 "	58	102	4 c. c. infus. digitalis.
12.42.15 "	58	70	
12.50.30 "	52	58	
12.53.30 "	55	50	
1. 8.30 "	62	76	
1.15.30 "	57	74	

Animal left up in a drooping condition till evening, when he was killed by a stab in the medulla.

*Exp. 10.*—Small rabbit struck by snake after a rest of six days.

TIME.	PULSE.	PRESSURE.	
2.0. 0 P. M.,			snake struck.
2.12. 0 "	70	75	
2.12.15 "	63	4 c. c. infus. digital.	67
2.12.30 "	63		94
2.12.45 "	70		90
2.13. 0 "	67		88
2.13.15 "	68		78
2.13.30 "	72		84
2.13.45 "	68		80
2.14. 0 "	70		84

TIME.	PULSE.	PRESSURE.
2.14.15 P. M.,	70	78
2.15.15 "	67	72
2.16.15 "	too faint to count.	84
2.17.15 "	—	90
2.20.15 "	4 c. c. infus. digital.	
2.21. o "	death.	

All these experiments seemed to show that infus. digitalis had the property to stimulate the main failure, a falling arterial tension.

When alcohol and digitalis were combined the results were about the same.

*Exp. II.* Rabbit struck by snake after a rest of four days, ecdysis having taken place two days previously, on Aug. 15, 1881.

TIME.	PULSE.	PRESSURE.	
1.20. o P. M.,			snake struck.
1.28. o "	65	50	
1.29. o "	68	50	
1.30. o "	71	48	
1.31. o "	68	46	
1.32. o "	67	46	
1.33. o "	66	44	
			1 c. c. tr. digitalis in 4 c. c. of water.
1.33.15 "	59	50	
1.33.30 "	59	50	
1.34.30 "	67	52	
1.35.30 "	74	102	
1.36.15 "	Pulse very feeble, one half c. c. of tinct. digitalis in 3 c. c. of water when pulse fell rapidly.		
1.38. o "	death.		

The following conclusions may be drawn from the experiments:

1. The rattlesnake poison mainly kills by producing a failure of the cardiac organ, and a great fall in arterial tension.

2. That ammonia, alcohol, and digitalis, temporarily increase the arterial tension.
3. That ammonia and alcohol increase the rate of pulse whilst digitalis slows it.
4. That toward the close of life the intravenous injection of either alcohol, ammonia, or digitalis stimulates the circulatory apparatus, but the excessive stimulation totally and rapidly exhausts the cardiac irritability. I purpose shortly to use the above agents first and then allow the snake to bite the animal, when I will consider the relation of the drugs more fully.

## SUICIDE IN EUROPEAN AND AMERICAN CITIES.\*

By ALBERT LEFFINGWELL, M.D.

“**S**UICIDE,” says Goethe, “is an event of human nature which, whatever may be said or done with respect to it, demands the sympathy of every man, and in every epoch must be discussed anew.” It is the paradox of conduct, the search for greater happiness by means which it is the very instinct of human nature most to dread. In every age it has furnished a text for religion, a theme for the dramatist, a subject for philosophy; but it is only within the present century that science has been able, by means of accurately gathered statistics, to measure its prevalence, to ascertain somewhat of the laws which govern its production, and to point out the methods by which we may hope to check its threatening tendencies. We may now know not merely the number of suicides which occur every year in each European capital or country; but we can distinguish the number of either sex, the causes which directly or indirectly impelled, the periods of the year, the presence or absence of family ties, the very methods they employed, and even the ages at which they grew tired of living and sought release.

To the American student, however, it is unsatisfactory to

\* Skene prize essay, read at the Annual Meeting of the Alumni Association of the Long Island College Hospital, June, 1881.

know facts of this character only with regard to Europe; he desires also to know how the love of life is affected by our Western civilization, under a government where all men are peers. "How," he asks, "does the rate of suicide in American cities contrast with its prevalence abroad? Is the annual rate of suicide in Brooklyn, Philadelphia, or New York greater or less than in London or Paris, Vienna or Berlin? Do influences of sex and race linger under the changed environment of the New World? Are the same methods in vogue in American cities as in Europe, and may we see any possibility of raising barriers against self-murder through legislation on the sale of poisons? Is the age of greatest tendency to suicide the same? Do family ties exert here the same influence? Does education impede or religion restrain?" These are questions to which a satisfactory reply is impossible from existing literature, nor to be had at less cost than a thorough personal investigation of the whole subject. This, the present writer several months ago determined to undertake. The labor has been far greater than he anticipated. Alone among civilized nations the United States has no exact record of its births, deaths, or marriages from year to year; nothing, as a nation, but the carelessly gathered facts of the census-taker collected once in ten years. Almost the only exception to this utterly unscientific carelessness are the health departments of a few cities. A satisfactory comparison of metropolitan suicide in Europe and America would have been absolutely impossible but for the generous assistance of members of the medical profession connected with these boards. From nearly every capital city in Europe to which inquiries were sent information was returned. Dr. Bertillon, "chef de la statistique de la Ville de Paris," and the most eminent statist in Europe: the Registrar-General of England; Dr. Berg, chief of the Statistical De-

partment of Sweden; Dr. Maurice Gad, of Denmark; Dr. Josephy, of Vienna; and Dr. Engel, of Berlin,—all these responded most generously to requests for detailed information in regard to suicides of their respective countries or capitals. To the health departments of several American cities (and especially to the registrars of vital statistics of Brooklyn and New York) the writer is under peculiar obligations. In the present essay we shall attempt to classify and contrast only a part of the information thus obtained.

#### FREQUENCY OF SUICIDE.

In order to obtain a fair conception of the prevalence of suicide, it will be necessary to view its occurrence, not for a single year, but for a period sufficiently extended to show that the numbers are due to no extraordinary combination of circumstances, but are of *regular occurrence*. It is needless to furnish a detailed abstract for all the cities we shall examine, and in the following table I have selected Brooklyn and New York, London and Berlin; and give herewith the suicides of either sex for a period of ten years.

SUICIDES, FOR A PERIOD OF TEN YEARS, OF EITHER SEX.

	BROOKLYN.		NEW YORK.		LONDON.		BERLIN.	
	Men.	Women.	Men.	Women.	Men.	Women.	Men.	Women.
1869 . . . .	—	—	—	—	229	78	—	—
1870 . . . .	—	—	79	22	196	85	116	56
1871 . . . .	18	10	85	29	207	86	119	29
1872 . . . .	23	8	111	33	196	74	101	39
1873 . . . .	22	9	82	36	211	66	115	52
1874 . . . .	30	4	140	40	189	68	156	48
1875 . . . .	29	10	123	32	199	98	166	46
1876 . . . .	41	10	114	36	221	74	183	65
1877 . . . .	35	15	123	25	202	70	231	73
1878 . . . .	41	11	116	26	225	93	251	72
1879 . . . .	30	7	100	17	—	—	251	107
1880 . . . .	26	5	—	—	—	—	—	—
Total by sex .	295	89	1,073	296	2,075	792	1,689	587
Both sexes .		384		1,369		2,867		2,276

We cannot help experiencing a feeling of wonder in the study of statistics like these,—not only at the average regularity with which men and women of such different surroundings fling away their lives from year to year, but also at the remarkable and uniform difference in this respect manifested by persons of different sex. No matter how widely, in the countries of Europe, the variance in the rate of suicide may be (and it is no less than thirteen times greater every year in Denmark than in Ireland), the proportion between the sexes is always about the same. Take the four cities we have just examined in detail; and of the total suicides for ten years, between 70 and 80 per cent. will always be men.

OF TOTAL SUICIDES, WHAT PER CENT. ARE MALES OR FEMALES?

	Male.	Female.	Both.
New York, 1870-'79 . . . . .	78	22	100
Brooklyn, 1871-'80 . . . . .	77	23	100
Berlin, 1870-'79 . . . . .	74	26	100
London, 1869-'78 . . . . .	72	28	100

A much better method of looking at this phenomenon is by comparing the proportion which suicides of either sex bear to the male and female population in the cities where they occur, and in the following table I have estimated this proportion for five European capitals and as many American cities.

My comments on this table must be necessarily briefer than its importance deserves. Several points should be noted by the reader. The first question naturally arising is, what degree of reliance can be placed upon these deductions?

TO 1,000,000 LIVING OF EACH SEX, HOW MANY SUICIDES ANNUALLY?

Period observed.	Date of Census, or Estimate of Population.	CITY.	AVERAGE ANNUAL NO. OF SUICIDES.		AVERAGE NO. OF SUICIDES ANNUALLY TO 1,000,000 LIVING.	
			Men.	Women.	Men.	Women.
1877, '78, '79 (3)	1876	Paris,	555	171	551	175
1870-'79 (10)	1875	Berlin,	169	58	349	121
1869-'78 (10)	1878	Stockholm,	37	8	439	91
1869-'78 (10)	1875	Vienna,	123	42	241	82
1869-'78 (10)	1872	London,	207	79	131	45
1878, '79, '80 (3)	U. S., 1880, $\frac{1}{2}$ and $\frac{1}{2}$ .	San Francisco,	74	11	699	110
1870-'79 (10)	State census, 1875.	New York,	107	29	211	55
1871-'80 (10)	Estimate of 1875.	Boston,	27	7	186	42
1871-'80 (10)	State census, 1875.	Brooklyn,	29	9	127	35
1869-'78 (10)	Estimate of 1875.	Philadelphia,	40	9	126	25

For Europe, they are as nearly accurate as possible. For the period observed, I have in all cases taken the census figures of population, when to be obtained, for the year nearest the middle of observed period. For Paris, the rate is probably a little higher than it should be, because estimated by the census of 1876, instead of an estimate of population for 1878. For Stockholm, for a similar reason, the rate is a little lower than it should be; say for both cities three or four units—too inconsiderable to affect general reliability.

San Francisco presents such startling and exceptional results that I have taken only three years and estimated

the number of suicides by the population in 1880, making even the high rate obtained less than it should be. New York and Brooklyn are very accurately measured by the State census. For Philadelphia and Boston I have taken the mean of the two U. S. censuses of 1870-1880 to be the probable population in 1875. Let us now see what deductions may be safely inferred.

Suicide, when measured by population, is not only invariably three to five times more frequent among men than among women, in Europe and America, but the causes which, in one city, increase its prevalence among men, heighten the rate proportionately (or nearly so) among the other sex. This is so true, that we could even calculate very nearly what the rate of suicide would be, under similar state of civilization and environment, were knowledge wanting. Suppose, for example, that we knew nothing whatever of the rate of suicide among the female population of Brooklyn, would it be possible to estimate it with any degree of accuracy? Certainly, if we know the facts for New York. The two cities lie side by side, and if any causes have tended to make the rate of male suicide lower in the City of Churches, we might be sure that they have proportionately influenced the female population. The problem would be an exceedingly simple one:

## RATE OF SUICIDE FOR MEN.

N. Y.      Brooklyn.

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## RATE OF SUICIDE FOR WOMEN.

N. Y.      Brooklyn.

: :      55      ?

and we should find our answer to be 33—only two less per million population than the actual statistical evidence of ten years. Why fewer women than men commit suicide is one of the questions to which reply cannot be satisfactorily given. It may be that everywhere, especially in our great cities, fewer women than men are subjected to the “slings

and arrows of outrageous fortune"; that upon her falls less frequently the poverty which disheartens, the keen anxieties and disappointments of commercial life, the temptations of intemperance, or the incitements to vice and crime. It may be that, on that lower level of destitution and misery, where man can see no refuge from beggary but in suicide, woman perceives too often a door flung open, and hands stretched out to welcome her, to longer but dishonored years. Or, perhaps, and more probable, it is because her moral nature is inherently stronger and better than ours that woman hesitates longer before temptation, that the promises of religion are more highly valued, and its restraining influences more surely effectual.

#### INFLUENCE OF RACE.

We see, too, that the rate of suicide in every European capital named is higher than in Brooklyn. In Paris the rate of suicide is nearly *five* times, and in Berlin almost *three* times higher for both sexes than with us. Even New York is exceeded by every city except London, whose rate of suicide very closely approximates that of Brooklyn. But even this exhibit is not as clear as we would like. If it be true that the suicidal tendency of certain races is very marked, the presence in our midst of a large element, foreign-born, might naturally be expected to do much in heightening our rate.

Now I am indebted to Dr. Wyckoff, of Brooklyn, and Dr. Nagle, of New York, for a record of the nationality, so far as ascertained, of all suicides in Brooklyn and New York, for a period of ten years. And during this time there is not a single year in New York (and but one in Brooklyn) in which the foreign-born suicides do not outnumber the native-born of either sex. The total results may be of interest:

## NATIONALITY OF SUICIDES, BROOKLYN AND NEW YORK.

(So far as ascertained.)

	1871-'80. BROOKLYN.		1870-'79. NEW YORK.	
	Men.	Women.	Men.	Women.
Born in U. S. (10 years) .	97	24	254	78
Foreign-born " "	198	65	819	218

But what is the proportion between these numbers and the native- and foreign-born population from which they arise? That is the question we need to answer, and the reply to which is somewhat difficult. The usual method of calculating ratios of this kind is by dividing the number produced by the population which produces; yet if we should do this, and should attempt to calculate rate of suicide to total population, native- or foreign-born, it is evident we should have reckoned among the "native-born" thousands of children—one, two, and three years of age, and so on—whose parents were foreign-born, and who ought, in justice, to be included with them or subtracted altogether. Suppose we do that for New York, and subtracting from the population all under 15, ascertain the ratio of suicide to the population thus remaining. The result would be as follows:

NEW YORK CITY.	BORN IN THIS COUNTRY.		FOREIGN-BORN.	
	Men.	Women.	Men.	Women.
Population over 15 in 1875. .	132,376	141,031	201,626	220,473
No. of suicides, 1874-'75-'76.	83	28	294	80
Average annual rate of suicides to 1,000,000 living—	208	66	486	121
over age of 15 . . .				

If these results may be accepted, it would seem that the tendency to suicide is about twice as strong with men and women of foreign birth as for native-born Americans. Yet

even here distinctions must be drawn. The rate of suicide among the English, Scotch, and Irish is even less than among Americans; and the high rate shown by the foreign portion of our city population is almost wholly due to the Teutonic and Scandinavian element composing it. Other questions press for solutions, as yet impossible to give. It may be that this higher rate of suicide with foreigners is partly due to more poverty and greater destitution; more of the circumstances which dishearten and tend to create despair. Perhaps far more found themselves deceived in their dreams of obtaining wealth or even a livelihood in our great metropolis; more were homeless, penniless, friendless, among strangers, and without the means of support. Still, after deducting all these contingencies, there can be no doubt that *race* is an important factor in the prevalence of suicide. Upon no other hypothesis can we explain the differences between Berlin and Stockholm, between Paris and London.

#### INFLUENCE OF AGE.

At what time of life do most suicides occur? Here, again, statistics vary somewhat according to race and sex, and other phases of environment. But no age is entirely exempt; the school-boy of less than a dozen years, and the aged pensioner of fourscore, have taken into their own hands the settlement of the question of continued existence. If we take the total number of suicides in London, Paris, and New York, and ascertain the rate per cent. which the suicides at given ages bear to the total for all periods of life, we shall get the result tabulated below. For Paris the official reports do not distinguish the sex of suicides as regards age, and we can show these differences only in the last columns.

## WHAT PER CENT. OF SUICIDES ARE AT VARIOUS AGES?

AGE.	MEN.		WOMEN.		BOTH SEXES.		
	New York.	London.	N. Y.	London.	N. Y.	London.	Paris.
15 to 35	35%	26	44	36	37	29	30
35 to 45	27	22	22	21	26	22	19
45 to 55	20	22	21	19	20	21	21
55 to 75	17	27 $\frac{1}{2}$	11	22 $\frac{1}{2}$	16	26	28
75 and over.	1	2 $\frac{1}{2}$	2	1 $\frac{1}{2}$	1	2	2
All ages.	100	100	100	100	100	100	100

This is a curious table. At only one period of life, between 45 and 55, is there any substantial agreement between these three cities. Yet between London and Paris there is far less difference than between either city and New York, as respects age of a majority of suicides. Do we, then, in America tire of living sooner than the Frenchman or Englishman? It would almost seem so, judging from the ages at which the higher percentages of suicides occur. In New York over 62 per cent. of suicides are under 45 years of age; while in Paris or London, the proportion is only one half. We can see, too, from this table that the impulse to suicide is proportionately far greater in youth with women than with men; and it is doubtless because the influence of disappointment in her affections is far stronger with the weaker sex.

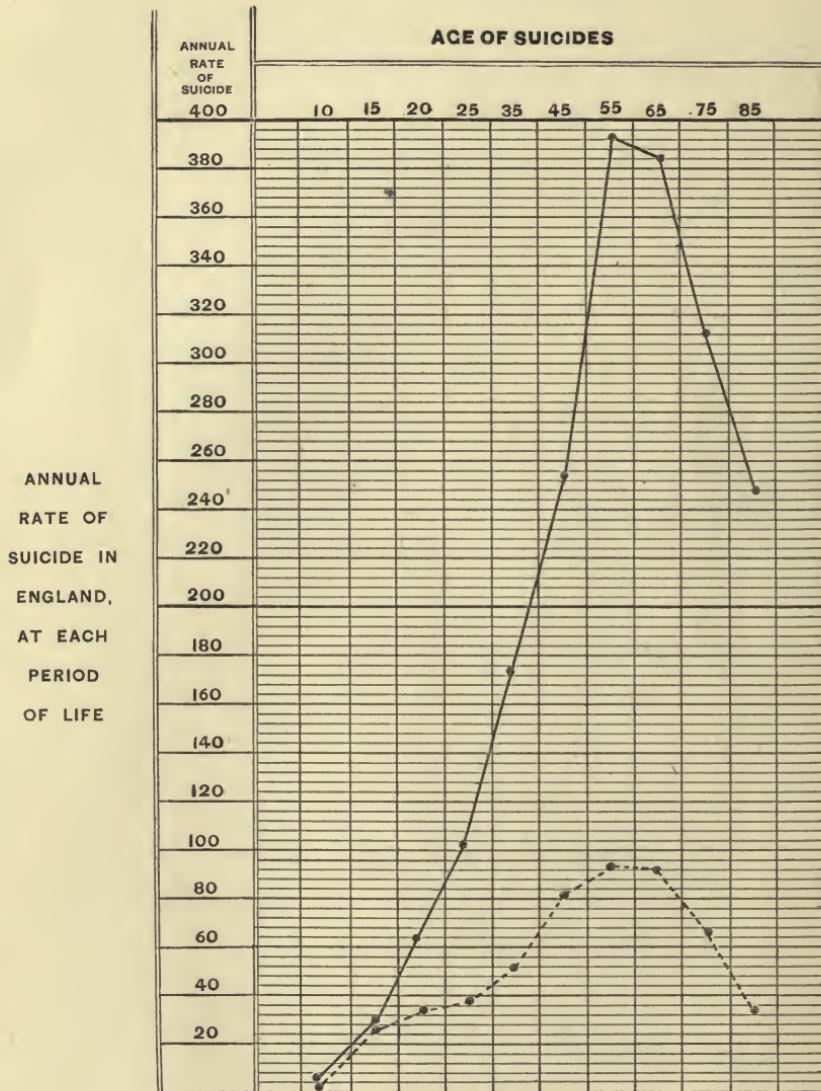
Still this is an unsatisfactory comparison; what we wish to ascertain is the proportion which the suicides at each period of life bear to the number living at the same age. But for cities, with their ever shifting and transient population, this is a very difficult, if not (at least for America) an

impossible task. If, however, we obtain the same facts for a nation, we shall obtain at least a clew to the facts as they probably exist in its cities and towns. I have, therefore, at considerable expense of time, taken the reports of the Registrar-General of England for ten years, 1867-1876, and ascertained the number of suicides at each recorded age, both male and female; and estimated what proportion the average annual number of these self-murders bears to the population living in 1871 according to the English census of that year. The results are sufficiently interesting to be included in the present study. To the final columns, however (in heavy faced type), the reader's attention is specially directed.

TO 1,000,000 LIVING AT DIFFERENT AGES, WHAT IS THE RATE OF SUICIDE, ANNUALLY, IN ENGLAND AND WALES?

AGE.	MEN.			WOMEN.			Proportion of male and female rates of suicide to each other at each separate age.
	Population at different ages, by census of 1871.	Number of suicides 1867-1876.	Rate per annum to 1,000,000 living.	Population of different ages, census of 1871.	Number of suicides 1867-76.	Rate per annum to 1,000,000 pop.	
10 . .	1,224,544	50	<b>4</b>	1,207,224	40	<b>3</b>	58 to 42
15 . .	1,079,775	294	<b>27</b>	1,109,854	294	<b>26</b>	51 to 49
20 . .	963,152	594	<b>62</b>	1,045,391	334	<b>32</b>	64 to 36
25 . .	1,594,513	1,593	<b>100</b>	1,756,437	643	<b>37</b>	73 to 27
35 . .	1,234,721	2,157	<b>175</b>	1,344,420	719	<b>54</b>	76 to 24
45 . .	965,711	2,456	<b>254</b>	1,038,224	840	<b>81</b>	76 to 24
55 . .	642,562	2,513	<b>391</b>	702,456	655	<b>93</b>	81 to 19
65 . .	356,357	1,375	<b>385</b>	411,233	378	<b>92</b>	81 to 19
75 . .	121,037	378	<b>312</b>	151,633	98	<b>65</b>	83 to 17
85 and over.	14,544	36	<b>247</b>	23,280	8	<b>34</b>	88 to 12
	8,196,914	11,446	Average 140	8,790,152	4,010	Average 46	

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NO. I

Now if we cast these results into the form of a diagram, representing to the eye the prevalence of suicide by age and sex, we may bring out the facts even more vividly than by tables. See plate, fig. 1.

We reach, therefore, the following conclusions respecting age in its relations to suicide :

1. Between ten and twenty years the rate of suicide is nearly equal with both sexes.

2. With both sexes (although in widely different ratios) it reaches its maximum between fifty-five and sixty-five; from sixty-five, steadily declines, remaining nevertheless, even at eighty-five and after, higher than in youth. A recent contributor to *Blackwood's*, writing upon suicide, says that "the number of suicides in proportion to population grows steadily through all periods of life, from childhood to old age. People go on killing themselves between nine and ninety in a constantly increasing progression." This, we see, is a mistake; the limit is reached before ninety, although the facts are sad enough as they are. They indicate that suicide is not principally the resort of the thoughtless, the weak-minded, the impulsive, or the lunatic; but that it is, as another writer affirms it to be, "the refuge mainly of those who are worn out in a bitter and hopeless struggle against accumulated ills."

3. No age, except that of infancy, is entirely exempt. Among the suicides in England during the ten years examined I discovered one case of a girl of five years! Dr. Allan McL. Hamilton [Johnson's Cyclopedias, art. "Suicide"] states that "the common time of life for this crime is the period between the twentieth and fortieth year; although cases have been known to occur among young children, and at the age of seventy." The impression thus given, that suicides at the age of seventy are somewhat phenomenal, is by no means correct. If the reader will turn to page 152

he will see that in England, during the ten years 1867-76, no less than 414 men and 106 women committed suicide after the age of seventy-five; while the annual average of suicides over eighty-five is between three and four! If we turn to cities we find that in New York there were, during 1870-79, 13 cases of suicide after seventy-five; that in London (1867-76) there were 59 suicides after that age, and in Paris, during only three years (1877-79), there were no less than 113 instances over seventy, of which 29 were between seventy-five and eighty, 8 between eighty and eighty-five, 4 between eighty-five and ninety, and one poor wretch, who refused to be longer neglected by death, and committed suicide between ninety and ninety-five!

#### INFLUENCE OF MARRIAGE.

We come now to an exceedingly interesting problem in our study. What effect does the presence or absence of marital responsibilities exert upon the tendency to self-destruction? Considering suicide as the expression of man's inexpressible distaste for longer existence, it is certainly worth while to know whether the felicities of marriage strengthen his attachment to life; or whether its cares and anxieties, and the unhappiness which, unfortunately, sometimes accompanies it, do not, on the whole, overbalance its restraining influences. In seeking the solution of this question it seemed to me better to go over the ground myself, rather than rest upon authorities; and letters were therefore addressed to the principal statists of Europe, asking returns of social condition of all suicides in their respective countries or cities. Curiously enough, English statistics, otherwise so valuable, could give no light whatever on this question; the Registrar-General, Sir George Graham, wrote me that not even for London could this information be obtained. From other sources, fortunately,

facts were gathered; and in the following table I am able to present the social condition of suicides, not only for three European capitals and as many countries, but also for ten years in New York, four years for Brooklyn, and three for San Francisco.

SOCIAL CONDITION OF SUICIDES COMPARED.

		SOCIAL CONDITION SO FAR AS KNOWN.			OF EACH 100, SUICIDES PER CENT.		
Period Observed.	Country or City.	Married.	Single.	Widowed.	Married.	Single.	Widowed.
1863-'75	France . .	28,677	21,766	10,616	47.	35. <sup>6</sup>	17. <sup>4</sup>
1869-'78	Sweden . .	1,832	1,410	469	49. <sup>4</sup>	38.	12. <sup>6</sup>
1870-'79	Denmark . .	2,432	1,388	947	51.	29.	20.
1869-'78	Stockholm . .	139	310		31.	69.	
1869-'78	Vienna . .	519	864	136	34.	57.	9.
1870-'79	Berlin . .	870	968	268	41. <sup>3</sup>	46.	12. <sup>7</sup>
1877-'80	Brooklyn . .	112	36	16	68.	22.	10.
1870-'79	New York . .	681	396	127	56. <sup>6</sup>	33.	10. <sup>4</sup>
1878-'80	San Francisco	101	139	25	38.	52. <sup>6</sup>	9. <sup>4</sup>

We have seen that percentage estimates are simply an approximation to truth, satisfactory only because in so many cases nothing else can be had. It would be very unsafe to assume—as the reader might be inclined to do—that because 47 per cent. of French suicides were married, and only 35 per cent single, marriage therefore increases in place of lessening the suicidal predisposition. Dr. Bertillon, of Paris, has kindly furnished me with the proof that, so far from this being the case, an exactly opposite conclusion

must be drawn, when statistics of social population are taken into account.

FRANCE, 1863-1875.

Social State.	Average Annual Population of France, 1863-75. Above legal age : Men, 18 ; Women, 15.	Total Number Suicides.	Average Annual No. of Suicides.	Rate to 1,000,000 Living, Each Status.
Married .	15,088,500	28,677	2,206	<b>146.<sup>8</sup></b>
Single .	8,180,325	21,766	1,674	<b>204.<sup>7</sup></b>
Widowed	2,894,575	10,616	817	<b>428.<sup>6</sup></b>

When we take population into our problem, we find, therefore, that bachelors and "spinsters" are far more prone to suicide than the married; and that the greatest predisposition is exhibited by widows and widowers. Similar results, Dr. Bertillon claims, have been obtained throughout Europe, wherever the statistics necessary for calculation have been at hand. Marriage, then, is more than a means of personal happiness; it is a benefit to society, an advantage to the State, to be encouraged in every way possible; a check to suicide and crime. One point, for which considerations of space forbid adducing proof, may be mentioned before leaving the subject. As a deterrent from suicide the influence of marriage seems to act upon women with far greater force than it exerts upon men. Perhaps this is only another proof that

" Man's love is of man's life, a thing apart ;  
'T is woman's whole existence."

Nevertheless it must be confessed that the statistics for New York and Brooklyn have a slightly suspicious look about them. Are there, then, so many more married persons in our stirring American cities as justify these proportions of suicide? In Brooklyn, for example, are there

above the age of 15, three times as many married persons as there are bachelors and maids, widows and widowers? Of the population of New York over 15, is only one third unmarried? At first thought it assuredly seems questionable whether the married population of our two cities is so much greater than its proportion in Berlin, Vienna, or Stockholm, and yet if it is not, then marriage fails to have with us that controlling influence which it exerts abroad. It is a problem for which the complete census returns will perhaps afford us means of solution at present denied.

One error, in an authority very frequently quoted, may be here pointed out. Dr. Forbes Winslow, in his "Anatomy of Suicide," says: "It has been satisfactorily established that among men two thirds who destroy themselves are bachelors." Now although, as I have shown, the tendency to suicide among bachelors is undoubtedly greater than among the married, there is no justification, so far as I can discover, for such a wide-sweeping statement as this. In Vienna, for example, the proportion of bachelors during ten years was a little over half the men, or 55 per cent.; in Berlin they were less than half, or 44 per cent.; in Sweden they were 38 per cent.; in Denmark but 30 per cent., or less than one third. Such statements as the one quoted make clear how wide from truth may be the highest authority when founded upon opinion instead of fact.

#### INFLUENCE OF SEASONS.

There can be no doubt but that the different seasons of the year sensibly and persistently influence the propensity to self-destruction. Contrary to what might be supposed, it is not the melancholy days of autumn, nor the gloom of winter, but the brightness of spring and summer which exert this effect. I pass by the evidence of this fact in detail, to bring forward a very singular result in relation to

this phenomenon. If we divide the year into two halves,—spring and summer on the one hand, autumn and winter on the other,—we find that not only do suicides preponderate during the more genial seasons, but also occur in a majority of cases the initiatory symptoms of insanity—(if statistics of first admissions to asylums can be fairly taken as evidence on this point, as I think they can). Still more strange is the fact that if we take the criminal records of a country like France, and separate the crimes against property (such as larceny, theft, burglary, etc.) from crimes against persons (rapes, violent assaults, attacks with intent to kill, from anger or revenge, etc.), and of these last-named ascertain

#### INFLUENCE OF SEASONS UPON SUICIDE, INSANITY, AND CRIME.

		Per 1,000 cases, proportions happening during		
		Spring and Summer.	Autumn and Winter.	All the Year.
<b>Insanity . . . . .</b>				
Esquirol's admissions to Charenton	{ 1,554 cases }	548	452	1000
Parchappe's " " Asylum .	{ 2,669 cases }	542	458	1000
Penn. Hosp. for Insane, . . .	{ 7,867 cases }	545	455	1000
<b>Crimes against Persons. France</b>	{ 6,475 cases }	535	465	1000
<b>Suicides : England (1817-1826)</b>	. . . . .	610	390	1000
" Paris (7 years, Dr. Winslow)	. . .	602	398	1000
" France, 1861-65, men	. . . . .	580	420	1000
" " " women	. . . . .	556	444	1000
" Westminster, England, 1812-1836	.	543	457	1000
" New York City, 1870-1879	. . .	553	447	1000
" San Francisco, 1871-1880	. . .	524	476	1000

the season of the year in which they happen, these also will be found most prevalent during the very months when suicide most prevails. In the following table I present such evidence of this phenomenon as I have been able up to this time to obtain.

These facts seem to me too numerous and uniform to be the result of mere accident, but rather as evidence of the theory that suicide and crimes of violence have something in common with insanity. It may be interesting, on this subject, to know that Quetelet, one of the first statists of our century, points out in his work, "Sur l'Homme" [page 111], that the documents of criminal justice in France make clear the singular fact that the months in which occur the maximum of conceptions, are also the ones in which the most attempts upon chastity are made, and he suggests that this coincidence makes it probable that criminals, in these cases, are carried away by an irresistible impulse. This, however, is a question which does not concern our subject to investigate or discuss at present. The whole question of crime has almost but just begun to attract the attention of students; and there are doubtless mysteries to be here fathomed—of which, to-day, we have little comprehension.

#### METHODS OF SUICIDE.

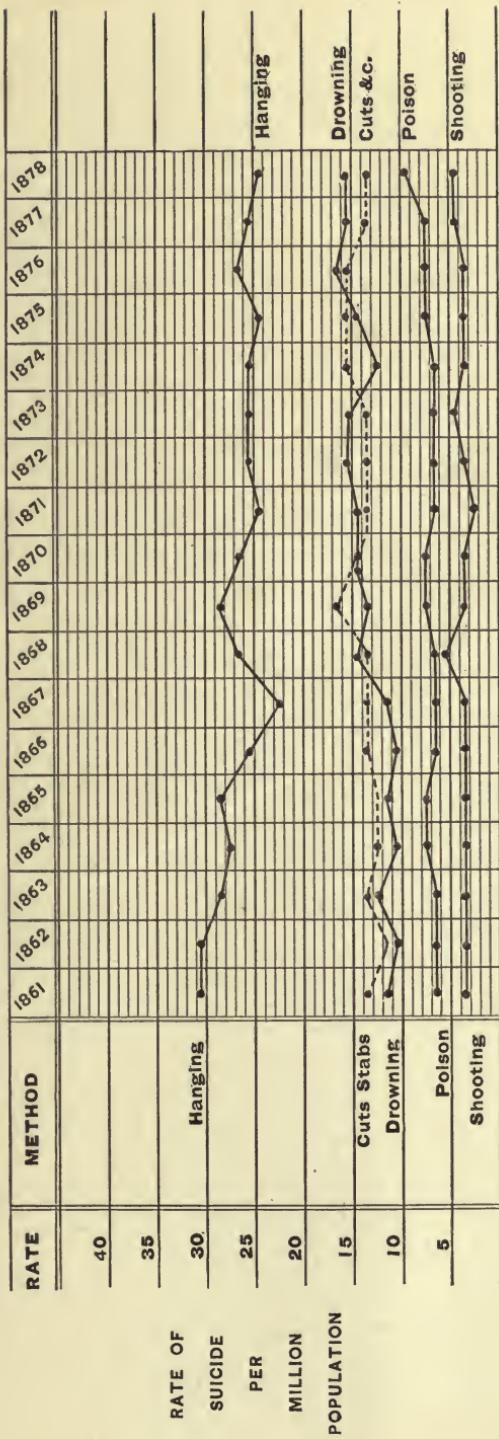
The methods selected by intending suicides offer another suggestive study. The uniformity with which, year after year, a fashion once established persists in claiming followers, has invariably struck statists from the first. In the whole realm of nature there is hardly a more wonderful phenomenon than this regularity with which men and women, under a given state of civilization, choose in about the same proportions, in succeeding years, the methods of their death. Take England, for instance. During

eighteen years, 1861-1878, we see by the reports of the Registrar-General that per million population of England and Wales, the number of persons who killed themselves by shooting was exactly three, for thirteen years, varying only during the remainder of the time from two to five; while those choosing poison were either six or seven for every year but the last. I have constructed from the available figures a diagram. See plate, fig. 2.

We see, then, that throughout this long period of many years the fashion of suicide has remained precisely the same, excepting only in a few instances to indicate a preference for water in place of lead.

It is a rather singular circumstance that so many writers on this subject, attempting to speak with authority, have fallen into very palpable errors. If we cannot repose entire confidence in our favorite cyclopædia, where may we turn, or in what authority shall we place our trust? Yet they have invariably erred in attempting to lay down a general and universal fashion to which suicides are supposed to conform. Both "Chambers's Cyclopædia" and the "Encyclopædia Britannica" (8th ed.), two of the highest authorities, name an order of suicide for both sexes in which errors exist. A writer in *Blackwood's Magazine* during the past year ventured to lay down a fashion for self-destructive methods, which is farther than either of the other named authorities from being correct. Some of these mistakes are quite incomprehensible and inexcusable for English writers, to whom the reports of the Registrar-General are accessible. I have taken the trouble to analyze the methods of suicide in England for men and women during ten years, and the figures (which I shall not here occupy space in giving) demonstrate, beyond question, the inaccuracy of these authorities. But it can be proven also by the facts for a number of cities, which are of more inter-

PER 1,000,000 POPULATION, NUMBER OF SUICIDE IN ENGLAND DURING 1861-1878, BY FIVE PRINCIPAL METHODS



NO. 2



est. I have taken simply the order of preference of both sexes, omitting the percentages, which only perplex the eye.

## IN WHAT ORDER ARE THE METHODS OF SUICIDE PREFERRED?

AMERICAN CITIES.					EUROPEAN CITIES.				
	New York.	Brooklyn.	San Francisco.	Philadelphia.	Berlin.	Vienna.	London.	Paris.	
1	<b>Poison</b>				Hanging <b>Poison</b>	Hanging <b>Poison</b>	Hanging <b>Poison</b>	Hanging	Asphyxia
2	Shooting	Hanging	Cuts		Shooting	Shooting	Cuts	Cuts	Hanging
3	Hanging	Shooting			Cuts	Drowning			Drowning
4	Cuts, etc.				Hanging				Shooting
5	Other methods				Drowning				Strangulation
6	<i>Drowning</i>				Other methods				Precipitation
7	—	—			—	—	—	—	Poison

Certain deductions from above facts may be stated as follows.

1. It is manifestly impossible for any authority, cyclo-pædia-writers included, to lay down a fashion in suicide which shall be generally applicable everywhere. Each locality or country preserves its own averages; and the error of writers on this subject has been to imagine an observed order for one country could be stated as applicable to all.

2. Very many suicides, in which design is not easily distinguished from accident, escape record altogether. This is so especially the case with regard to drowning, that it vitiates very materially any accurate comparison between our American cities and those of Europe. I believe it is the rule in New York to count no such death as suicide unless intent is proven, or the act seen; the rest are merely "found drowned." How far from truth this must be we can readily imagine from the experience of a single year. In 1879 no less than 138 dead bodies were taken from the waters surrounding New York, of which 39 were never identified. How many suicides did the coroner ascribe to "drowning" in that year? Seven.

3. The above tables demonstrate that poison is more generally used by suicides in America than in Europe. The reason for this is undoubtedly the greater facility with which it may be procured. In San Francisco, Dr. Dow, the present coroner, states that "no restrictions are put on the sale of poisons, any one at any time being able to obtain from any drug-store all he may desire." What is the result? Between 1875-80, of all the female suicides sixty-five per cent.—nearly two thirds—were by poison! In New York, between 1870-79, fifty-three per cent. of female suicides were by poison.

#### INFLUENCE OF FINANCIAL DEPRESSION.

The connection between a high rate of suicide and a





general depression in trade and industry is so striking that it deserves remark. Its effect is, of course, remote rather than immediate ; it lowers wages, lessens the opportunity of obtaining a livelihood, increases the poverty of those already poor, ruins the small trader, and thus everywhere depresses the tendency to hopefulness and the love of living. In the accompanying diagram (plate, fig. 3) I have shown the rise and fall in actual numbers of suicides in several cities of this country and Europe during a period of ten years. Its value would have been greater could we determine the rate per million population for each year and each city ; but this unfortunately is impossible.

A slight rise should be expected from year to year, but the decrease is only explicable on the ground that the previous rise in number was greater than the addition to population. The facts for American cities only can be brought out in a table giving actual numbers of suicides for each year, 1872 to 1879. The greatest number of suicides I have written in black, especially to distinguish it to the eye.

NUMBER OF SUICIDES IN AMERICAN CITIES.

	New York.	Philadelphia.	Boston.	Baltimore.	Brooklyn.	San Franciso.
1872	144	48	22	9	31	37
1873	118	47	26	24	31	59
1874	<b>180</b>	59	16	20	34	60
1875	155	<b>68</b>	<b>39</b>	<b>30</b>	39	56
1876	150	60	27	21	<b>51</b>	75
1877	148	59	34	19	50	<b>103</b>
1878	142	40	31	27	<b>52</b>	86
1879	117	—	28	15	37	90

This table brings into prominence the following points :

1. A wave of suicide passed over this country between 1872 and 1880. The highest level was reached for most Eastern cities in 1874 and 1875. Brooklyn is a marked exception, although the deaths by drowning in 1876 may have been more than in 1878, and so made the suicides of 1876 the majority. In San Francisco the maximum was not reached until 1877, and the depression which prevailed in the east did not touch the Pacific to any great extent until that year. It appears now to be everywhere receding with the general revival in trade and industry throughout the country.

2. In Europe there is a marked contrast between different cities in this respect. In London, the tendency, until 1874, was, on the whole, downward; but even in 1878, in proportion to population, the rate was lower than in 1869. In Vienna, on the contrary, up to 1874, there was a steady rise in the rate of suicide; but since then an almost steady decline. Berlin, however, from 1872, has shown an enormous increase in suicides, out of all proportion to population. The people are discontented with their government, unsettled in their faith, and, suicide is the simple and legitimate expression of discontent with one world and despair of another.

Here we must end the present study. It would doubtless be of interest to continue the investigation into other fields, to review the various causes which excite or impel the act, to note how far suicide is the result of insane delusion or correct judgment, of selfishness or altruism, of vicious or virtuous lives, of the fear of shame and disonor, or the pangs of disappointed love. We need also to know to what extent it springs from causes which society might alleviate: from want and destitution in old age, from lack of sympathy in distress, from the discomforts and

pains of disease, which, even when incurable, might be rendered less. It would be valuable to study the effect of religion as a restraining influence, and curious to note whether we might discern any difference in the suasion of diverging creeds. But for due treatment of these topics space is wanting ; it must suffice to have imperfectly pointed out a few facts not generally known, and—viewing suicide as a social disease—to have indicated some points in “diagnosis,” leaving for future consideration elsewhere the methods by which, one day, we may hope to restrain impulse, to arrest tendency, and prevent despair.

## THE TREATMENT OF WINE-MARK BY ELECTROLYSIS.\*

BY GEORGE HENRY FOX, M.D.,

CLINICAL PROFESSOR OF DISEASES OF THE SKIN, COLLEGE OF PHYSICIANS AND SURGEONS,  
NEW YORK.

TWO years ago I had the privilege of making some remarks before this Society on three methods of treating the superficial nævus, or "wine-mark." These methods were: by linear scarification, as recommended by Mr. Balmanno Squire, of London; by puncture, or so-called "tattooing," as recommended by Dr. Sherwell, of Brooklyn; and by electrolysis. The success which has attended the use of electrolysis in the removal of superfluous hair, first led me to think that the electrolytic needle might be used to advantage in the treatment of wine-mark, by a subcutaneous destruction of the capillary vessels. I did not start with the idea that this plan of treatment would remove a wine-mark and leave the skin in a perfectly normal condition, but it occurred to me that by creating numerous minute cicatrices upon the surface of the patch the color could be so reduced that the mark would present little contrast with the surrounding skin. At the same time there would be no danger of producing a smooth scar of a dead white hue and contractile character, which would be scarcely less disfiguring than the original deformity. At the meeting

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\* Read before the N. Y. State Medical Society, Feb., 1882.

of the Society referred to, I suggested this operation, and mentioned a case in which I had resorted to it with partial success. To-day, with more experience in the treatment of this affection, I can recommend electrolysis as being, in my judgment, the best method, except for the mildest cases, in which a simpler plan will suffice. The object aimed at in electrolysis, as in scarification and puncture, is to excite sufficient inflammation to destroy the fine network of blood-vessels. As the galvanic current is more active, and, at the same time more manageable than acid adhering to the point of a needle, it is not strange that this method should produce the desired effect in the speediest manner and with the least injury to the surface of the skin.

The operation is quite similar to that which I have already described in a paper read before this Society, on "the permanent removal of hair by electrolysis." A single needle, or an instrument containing a dozen or more needles with points upon the same plane and about two millimetres apart, is attached to the negative cord of a constant-current battery. If these needles are fine and with sharp points they can be quickly pressed into the skin without inflicting much pain, and the electrolytic action which takes place around them as soon as the galvanic circuit is complete, serves to destroy the capillary network

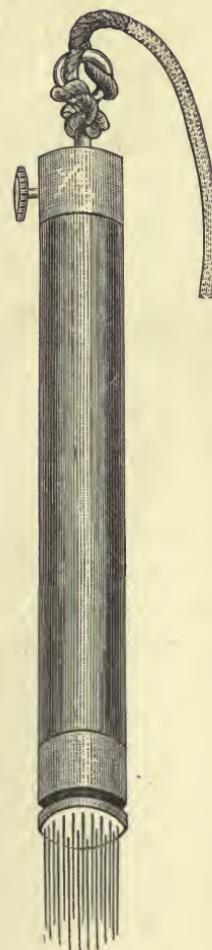


FIG. I.

upon which the existence of the wine-mark depends. This it does with but slight injury to the skin itself. The instrument which I have employed is a small brass disc, with from four to twenty holes, through which are crowded fine cambric needles. This disc (fig. 1) screws on a handle about the size of a short pen-holder. When the needles are introduced into the affected skin and the patient completes the circuit by grasping a moistened sponge electrode attached to the positive cord, a blanching of the tissue for a small space around the needles is immediately observed. With ten or fifteen cells of an ordinary zinc and carbon battery in use the needles should be allowed to remain in the skin from ten to thirty seconds, the exact time depending upon the delicacy of the skin and the effect observed. The blanching of the skin around the needles disappears in a few minutes, and nothing is seen but the punctures. In a day or two a group of dark pits or minute crusts are observed where the needles were introduced. In about three weeks the effect of the electrolysis becomes manifest, although the change may be so slight as to indicate a repetition of the procedure. In a case where a dark patch has existed, and the instrument has been repeatedly employed, the change consists in the transformation of color from purple to dark red, and through varying shades of red and pink to a light orange. At the same time the surface of the patch, upon close inspection, will be found to be covered by minute whitish dots. These are the cicatrices caused by destruction of tissue at the numerous points where a needle was introduced into the skin, and are of so slight a character that they are only apparent upon close inspection.

In every case of wine-mark of a severe grade, characterized by a deep purplish color and a tendency to the formation of angioma or small venous excrescences, the use of the multiple needle instrument, in the manner which I have

suggested, will certainly produce a most marked improvement in the appearance of the patient in a few months or even weeks. It will not suffice, however, for a complete removal of the mark. Indeed, where there is dilatation of the deeper plexus of blood-vessels, as is frequently the case in an aggravated form of the affection, there may be a slight tendency toward a return of the purplish hue, after this has been removed by the operation. In such cases it is advisable to employ a very fine and flexible steel needle, and to introduce this in an oblique direction beneath the skin to the depth of a centimetre or more. By this plan it is possible to destroy some of the larger vessels through which the blood flows in its passage to and from the superficial capillary network. In this connection it will be remembered by some that Mr. Balmanno Squire, in his first publication on the treatment of wine-mark by linear scarification, advised that the incisions be made perpendicular to the surface of the skin. Later, however, he found that much better results could be obtained by making oblique incisions.



FIG. 2.

In an extensive wine-mark of the face involving the eye-lids the single needle (fig. 2) must necessarily be used, and I have been surprised and pleased to note the decided change in the expression of a patient's eyes when the dark

color of the lower lid and malar region has been removed. In a case where the upper lip was involved by the naevus and projected considerably at the angle of the mouth, a diminution of size was effected by piercing the lip with a fine electrolytic needle. But the injection of a few drops of pure carbolic acid beneath the mucous surface of this lip proved to be a more effective and less painful mode of treatment.

In recommending electrolysis in the treatment of wine-mark, it is but just to speak of its objectionable features. In the first place, the operation is a somewhat tedious and painful one, and consequently not adapted to the treatment of children who are not old enough to be annoyed by the disfigurement, and who are therefore unwilling to suffer a little pain for the sake of its removal. I have experimented with local anæsthesia, but have failed to find it of any particular service. Very likely others might have better success with it. In the second place, there is a slight danger of causing suppuration and superficial sloughing of the skin in some cases, and a tendency in other cases to the formation of small outgrowths of a keloidal appearance. In one case I was quite annoyed at an unexpected ulcer, of the size of a split pea, which resulted from my treatment, and left a slight depressed scar upon the cheek; and in two cases trifling ulceration was followed by the development of small, firm, vascular nodules. These were suggestive of keloidal growth at first, but they disappeared in the course of a few months, leaving the skin perfectly smooth. Some who have attempted the removal of wine-mark by Squire's method have reported failure and the development of keloid as the result of their endeavors. Whether these resulting excrescences were really keloidal growths is a question, but the tendency to the formation of vascular nodules, before as well as after operations in

cases of wine-mark, has been noted, and in using electrolysis the production of even minute ulcers should be carefully avoided.

Finally, I would remark that in recommending a new method for the removal of wine-mark, I feel chary of speaking too confidently of its merits, bearing in mind the fact that other methods of treatment, which have been recommended to the profession with more or less ardor during the past ten years, have failed to stand the test of time and trial at other hands. I do not believe it is possible to remove a wine-mark and leave a perfectly normal skin, but I do assert that the most unsightly and disfiguring patches can be greatly improved, if not entirely removed, by the production of numerous punctiform cicatrices, so small as to be scarcely noticeable save upon close inspection. I do not claim that the operation which I have practised accomplishes all that could be desired, but I am sure you will agree that a brilliant and most satisfactory result is accomplished, if we do no more than transform a dark and unsightly stain into a smooth patch of a light pink hue.

As a summary of my experience, I may state briefly that wine-mark is not, as is commonly imagined, beyond the reach of surgical skill; that the most unsightly and disfiguring cases can be greatly improved, if not entirely removed; that in few, if any cases, can the mark be taken away without leaving faint scars, or, at least, a change in the character of the skin; that electrolysis, in my judgment, furnishes the best means of treatment, and that it is especially adapted to those cases in which the mark is more or less venous in character and of a dark purplish color.

THE APPLICATION BY INSUFFLATION OF MEDICATED POWDERS TO THE UPPER AIR-PASSAGES FOR THE RELIEF OF CATARRHAL CONDITIONS.\*

By D. H. GOODWILLIE, M.D.,

NEW YORK CITY.

**T**O the general practitioner the local treatment of catarrhal conditions of the upper air-passages too often becomes an irksome duty.

The reason no doubt lies in the fact that when such conditions excite attention there is no efficient means of local treatment. Chronic naso-pharyngeal catarrh, in the majority of cases, has its beginning in early life and comes under the observation of the general practitioner.

A rhinitis, neglected in childhood, with the accompanying hyperplasia, resulting in hypertrophies of the tissues, and followed by the consequent malformations of various kinds, preventing normal respiration and creating naso-pharyngeal trouble in adult life. Hence the importance of early treatment, and so claims the attention of the family physician.

He may not be able to treat all cases, particularly those of a chronic character, as well as an expert, but there is no doubt that with the proper attention early given, with some efficient means, both local and general, he will not only give much relief, but prevent trouble in adult life.

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\* Read before the State Medical Society at Albany, Feb. 9, 1882,

But I cannot trespass on your time to give here the etiology of naso-pharyngeal catarrh, however interesting it may be, or to speak of general treatment.

My object at this time is to engage your attention to an efficient means of local treatment for general use.

I claim no originality in it, only to make the means a little more efficient to bring relief through your ministrations.

This local treatment consists in making application of finely triturated medicated powders by means of an improved insufflator.

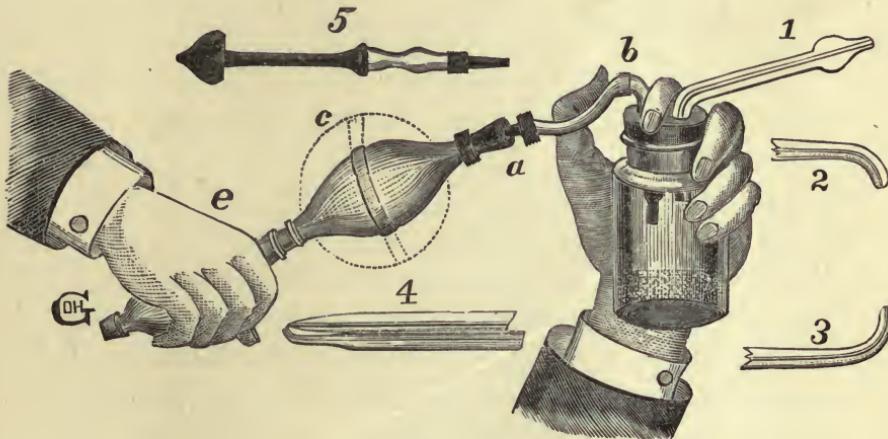


FIG. I.

The insufflator, fig. 1, is in principle the same as the one brought to the attention of the profession some time since by Dr. A. H. Smith, of New York. Glass tubes being now substituted instead of hard rubber, with valves inside the bottle, so that it is closed when not in use.

It consists of a bottle—that can readily be held in the hand—which contains the medicated powder. Through the cork pass two thick glass tubes with valves inside the bottle, one of which (1) conveys the powder from the bottle and has an enlargement on its end to fit the vestibule of the

nostril. Two other tubes are also made use of to make application to the larynx (2) or posterior nares (3). The other tube (6) passes also through the cork and has a valve on the end within the bottle. The other end is attached by rubber tubing to the rubber balls that supply the air. The rubber balls are readily joined and disjoined to the bottle by a hard rubber attachment ( $\alpha$ ).

The air is supplied by two rubber balls ( $e$ ,  $c$ ) closely joined together with a valve between them. One ball ( $e$ ) is worked in the hand, and forces the air into the other ball ( $c$ ), which can be distended according to the amount of air-force required.

The calibre of the bottle and the glass tubes are suited to the capacity of the air-balls.

When an application is to be made, take the bottle in the hand, and with the thumb press firm on the rubber tube over the end of the glass air-tube (6), so as to close entirely the passage of any air into the bottle. Now force by the hand the air in the rubber ball ( $e$ ) into the ball ( $c$ ), and distend it according to the amount of air-force required. Before an application is made, the nasal cavity should be as thoroughly cleansed as possible, then the powder blown in. The first application will cause an increased secretion from the muciparous glands, and possibly excite a sneeze. Then the nose should be blown, after which the powder should again be effectually applied.

To make an effectual application to the nose, pharynx, and larynx, it may be done in the following manner.

Distend the air-ball ( $c$ ) of the insufflator, then direct your patient to shut the mouth and exhale through the nose; the moment he ceases to exhale apply the nozzle of the tube into the nostril, then direct the patient to inhale; at the same instant make the insufflation, and the whole surface from the anterior nares to the larynx has an application of the powder.

If there is any stenosis in the nostrils, this cannot be so well accomplished, and it will become necessary to make the application post-nasal also, by using the proper tube (3).

When an application is to be made to the larynx only use the down tube (2).

In the same manner spray can be used if desired, but more force will be necessary (4).

The tube 5, fig. 1, is for inflating the deeper ear passages, and can also be used to blow mucus from the posterior nares when there is stenosis in the anterior nares.

#### IMPALPABLE MEDICATED POWDERS.

The success of local treatment lies in a great measure in the properly prepared therapeutic agents and their effectual application. An impalpable medicated powder will be found of great value for general use, more especially for the nasal cavity.

But very much of its therapeutic value lies in its proper trituration. Extremely minute division is absolutely necessary to obtain good results.

When proper trituration is attained the impalpable powder when blown into the air floats like smoke, or when blown into the air-passages it passes into every part.

The valuable gums employed in some of the powders cannot be effectually triturated in small quantities and retain their therapeutic value.

I have found that to do this properly it is much better to triturate it slowly in large quantities and at a cold temperature. This prevents frictional heat, which has a tendency to impair the therapeutic value of the gums especially.

The following powders have been found most useful for general use.

	NO. I.	gm.
R	Benzoini,	3 i—4.
	Morphine muriat.,	gr. vi—0.35
	Bismuthi sub-nitrat.,	}
	Potassii nitrat.,	} aa 3 ss—15.

Valuable for its sedative action. To be used in hyperæmic conditions with pain. In the beginning of an attack of rhinitis coat the mucus surface with it.

	NO II.	gm.
R	Aluminis,	3 i—4.
	Acaciae,	
	Bismuthi sub-nitrat.,	} aa 3 iv—15.
	Potassii nitrat.,	}

Useful where a strong astringent is indicated.

In case of hemorrhage from the nose, remove all the clot and immediately blow in this powder abundantly until the bleeding ceases.

	NO. III.	gm.
R	Iodoformi, {	aa 3 i—4.
	Camphoræ, {	
	Bismuthi sub-nitrat., {	} aa 3 iss—48.
	Potassii nitrat., {	

A good antiseptic.

To be used where the discharges are fetid, or where ulceration is present, or an excessive amount of granulations.

The camphor masks the odor of the iodoform.

These powders when impalpable and with the therapeutic integrity of the drugs preserved can be more effectually applied to the nasal passages than spray, and their good effect is certainly more prolonged.

For the general practitioner they are vastly more convenient than sprays.

The insufflators are made by P. H. Schmidt, of Broadway and 34th Street, New York, where also these powders can be obtained by the ounce. A set of three insufflators with nasal speculua and a tongue spatula can be had in a convenient portable case.

## THERAPEUTIC CONTRIBUTIONS.

### IV.

ON THE EFFICIENT DOSAGE OF CERTAIN REMEDIES USED IN THE  
TREATMENT OF NERVOUS DISEASES.\*

BY E. C. SEGUIN, M.D.

M R. PRESIDENT AND GENTLEMEN: I have been led to prepare this paper by the following consideration. I frequently see uncured cases of nervous disease for which the attending physician has prescribed the proper remedy, but having exhibited it in doses which, though justified by medical authorities, were wholly insufficient to influence the disease, he has failed. This has been more especially true of chorea, of cerebral and spinal syphilis, of certain neuralgias. In these cases the physician had been wanting in the experience and in the courage necessary to fight his way through opposing tradition and book-authority to success.

There are several evident causes for this timidity, which in a negative way is nearly as injurious to the patient as too great rashness would be.

In the first place, the influence of teachers in medical schools and of writers of text-books is thrown in favor of small or medium doses. Few if any teachers or writers take special pains to indicate the maximal doses of potent drugs;

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\* Read before the Medical Society of the State of New York, Feb. 7, 1882.

they teach in a condensed form, and with an eye to the safe training of students. This is very well as applied to students, but a time comes when a physician in active practice wants to know just how much physiological effect he can obtain with certain remedies without positively endangering his patients' lives. In the present state of our medical literature, unless he have time and opportunity to hunt through the files of the leading medical journals for detailed observations, or to read monographs on experimental therapeutics, he must work out his maximal doses for himself at the cost of much time, of some anxiety, and of not a few failures.

It seems to me that works on therapeutics intended for the practitioner should give, for each important, physiologically active remedy, a paragraph on maximal doses, clearly indicating the amounts necessary to produce the physiological effects (on man), which are often inseparable from remedial effect. These data should be taken from monographs and special articles on the subjects by men who have had experience in the use of the drugs mentioned. For I take it as granted that it is now just as impossible for one man to give us a satisfactory, practical work on therapeutics, as it is for one to produce a uniformly excellent work on the practice of medicine.

In the second place, I have observed that many capable druggists are alarmed at doses of certain remedies which are not only harmless, but essential to success. I clearly remember that, when a student, I heard the late Prof. Freeman J. Bumstead relate, with a mixture of amusement and anger, how a leading druggist had sent to him to inquire if he really meant to give 3iss of bromide of potassium at one dose. This was twenty years ago. Yet, only a few days since, a patient told me that her druggist told her that she must have a very strong stomach to stand such powerful medicine (she was taking gm. .004 of biniodide of mercury

and gm. 3. of iodide of potassium three times a day, and under this in one week had lost nearly all her syphilitic pain). Very frequently have I had prescriptions for my usual doses of Squibb's conium returned for revision by the careful pharmacist. I intend nothing derogatory by these remarks, for druggists are supposed to know only the maximal doses of remedies as given by books, and they but do their duty in sending a prescription back for revision, if any thing in it seems wrong. For my part, acknowledging a liability to error, I am always glad to see this healthy doubt applied occasionally to my prescriptions; yet I would not have physicians allow themselves to be influenced by the remarks or practice of druggists. Philosophically the two professions are absolutely separated: the one furnishes the other with the proper implements of treatment in the best possible condition; and it is the function of the physician to determine by scientific knowledge and by experience how, when, and how much these implements shall be employed. In more senses than one the physician is responsible for the dosage of remedies.

In the third place, it has seemed to me that our large manufacturing drug-firms exert a baneful influence upon therapeutics. They have flooded the country with formulas and ready-made compounds, and thus relieved the physician of the necessity of exerting his power to extemporaneously devise the compound required for the individual patient before him. Increasing numbers of physicians, instead of adapting the *materia medica* to their patients, practically adapt their patients to an already-prepared stock of elixirs, pills, and mixtures. It is so convenient to order one of these, so much easier than to weigh the indications presented by the case, to estimate the patient's susceptibility, and then to write out a good prescription for the case, or more exactly speaking, for the patient.

I propose to briefly review the posology of a few drugs—giving the doses as stated by the best authorities, by writers on therapeutics, and by clinicians, and then stating the doses which I believe to be useful and safe.

I wish it particularly understood that in advocating larger doses of these remedies I do so only on the basis of a tolerably large experience, and not at all from any theoretical scientific considerations. At the same time that I advocate efficient doses, I am carefully observant of all the circumstances which render patients susceptible, and always make an allowance for idiosyncrasy. Thus, in first prescribing a potent remedy, I take into consideration the age, sex, and size of the patient; and also make an estimate of his general condition, and note particularly the state of his circulatory organs. Then, for a patient whom I see for the first time, I order very small doses, doses such as the books justify, and by steady increase feel my way, fearlessly because watchfully, to the larger doses, often seemingly dangerous doses, which really affect the organism and may cure the disease.

In this matter I make no claim to originality, and would not affirm that the doses I recommend are always essential to success; I simply sum up my experience and place my results at your service.

#### I.—FLUID EXTRACT OF CONIUM.

(*Extractum conii fructus fluidum*.—U. S. P.)

Doses as given by authorities on therapeutics and *materia medica*:

Is not mentioned by STILLÉ and MAISCH, by STILLÉ, by NOTHNAGEL, and by GUBLER.

WOOD. Therapeutics (1880), p. 371. Dose,  $\text{mL}$  1 to 2. (!)

BARTHOLOW. *Materia Medica* (1880), p. 409. Dose,  $\text{mL}$  2,  $\text{mL}$  5, increased to  $\text{mL}$  40.

RICE. Posological Tables (1879), p. 28. Dose, from 3 to 5 minims, to be increased with caution.

Doses as given by clinicians :

Conium, in the form of fluid extract, is not, to my knowledge, mentioned by any standard writer on the practice of medicine.

MEIGS and PEPPER, Diseases of Children (1870), p. 505, article chorea, refer to Dr. J. Harley's doses of succus conii with apparent astonishment.

To Dr. JOHN HARLEY (The Old Vegetable Neurotics, London, 1867) we owe the present rational or physiological use of conium. He swept away the former traditions of the potency of the drug, and showed that most of its preparations were inert. He obtained definite physiological and therapeutical results from the succus conii, administered in doses of from 3 ii (gm. 8) to 3 i (gm. 32). By means of these quantities he obtained the paresis of third nerves, arms and legs, which is the characteristic result of conium action on the spinal cord.

The prototype of our excellent officinal preparation, the fluid extract made by Dr. Squibb, was unknown to Dr. Harley until just as his book was going to the press (p. 94, note).

Dr. Squibb and Dr. Manlius Smith had, however, read a paper before this Society, at its meeting in 1867, entitled : "An attempt to answer the question, Which part of conium is the best for medicinal use?" (See transactions of the New York State Medical Society for 1867.)

Ever since, we, on this side of the Atlantic, have possessed by far the most reliable and the most powerful preparation of conium; but I am sorry to add that it has been used rather inefficiently, and that even intelligent physicians are afraid to use the only doses which have any effect.

I have used conium a good deal in the last ten years, and have always employed the fluid extract as made by Squibb. I have tried it in chorea, in spasm of paralyzed limbs, in general irritability, and in insomnia.

When the indication is present, as in chorea, to obtain muscular relaxation, after a few tentative doses of 20 and 40 minims, I give at one dose 60, 80, or even 100 minims. These doses cause drooping of the upper lids (sometimes diplopia) and paresis of the arms and legs. I do not repeat the dose until after all the effects have passed off—in from 12 to 24 hours.

In a case of chronic adult chorea of 14 years' standing, which I almost perfectly cured in 1872-3, at the Epileptic and Paralytic Hospital on Blackwell's Island, a large part of the result (a very remarkable result in my experience) was attributable to paresis daily produced by a teaspoonful of Squibb's extract of conium for a month or more.

Many cases of insomnia, with wakefulness in the first part of the night, more especially those with fidgets or physical restlessness, are very much benefited by conium. I usually give 20 minims, with 20 grains of bromide of sodium in camphor water, at bedtime, to be repeated if necessary. In some cases (male adults) I give 50 or 60 minims at one dose in the mixture, not to be repeated. Such a sleeping-draught prescription has been repeatedly returned to me by druggists, because they thought the dose enormous. Indeed, I usually warn patients that the druggist may comment on the dose.

If we have a clear indication to give conium, we ought to give enough to fulfil the indications, and this cannot be done without obtaining the physiological effects. With due precaution, there is a wide and sure distance between physiological and toxic effects, yet, with reference to remedies

such as I shall referr to, how few physicians understand and appreciate that the curative effects are obtained in just that interval between physiological and toxic effects. To be successful we must be bold, as bold as physiological knowledge can make us, and yet as cautious in the first giving of powerful drugs to a patient as if we had no courage at all.

(*To be continued.*)

## EDITORIAL DEPARTMENT.

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### A SECOND YEAR'S EXPERIENCE WITH NON-RESTRAINT IN THE TREATMENT OF THE INSANE.

In 1794 Pinel removed the chains from the insane patients in the Bicêtre. At about the same time, or a little later, the Retreat at York was built and placed under the superintendence of William Tuke. Here the patients were treated with very little restraint ; but neither of these went so far as to entirely abolish restraint apparatus ; it was reserved for Charlesworth to attempt and for Gardiner Hill to carry it out at Lincoln, to be followed by Conolly at Hanwell, who confirmed its practicability and advantages on a large scale. Hanwell had at this time 800 patients. It is of interest to follow the various arguments which were then used by superintendents and others against the non-restraint system, and the numerous misrepresentations which were made as to the use of non-restraint by Dr. Conolly ; but in spite of these unjust criticisms, which were either intentional or due to that peculiarity of the human mind which causes it to oppose whatever is new, one by one the English superintendents adopted it, and to-day in Great Britain it is the almost universal practice.

The superintendents of American asylums have almost unanimously decided to defend the use of restraint apparatus, and to-day they use the same arguments and derisive sneers against non-restraint as were used in England in the days of Conolly. They fail to see that these arguments availed nothing in opposing the spread of the non-restraint system in Great Britain.

These superintendents say of an asylum or asylums which carry out non-restraint, "That they do not do it ; it is a myth" ; and feel perfectly satisfied that they have demolished the whole practice of non-restraint and its advocates.

I cannot avoid quoting from Conolly, for his remarks are so pertinent and applicable, and give exactly the arguments used to-day by superintendents. He says : "One general error seems also to pervade the minds of those who most severely condemn the abolition of restraint ; they always assume that if one kind of violence is discontinued, some other kind of violence is substituted for it."

They also affirmed that the patients were locked up in cells, etc. It was at this time also that originated among these opponents of Conolly the appellation of "chemical restraint," and which they claimed was the substitute for mechanical restraint. To-day, as then, the "chemical-restraint argument" is being constantly used. If non-restraint is and can be carried out in one or more asylums, that fact is sufficient answer to all objections of quibblers. And, if it is carried out, who could possibly question which is the better, the asylum with non-restraint, or the one with restraint ?

It is now two years since the adoption of non-restraint in the Kings County Insane Asylum, and from the experience during that length of time, the following has been found to be the difficulties to be surmounted and the advantages to be gained by carrying it out. The difficulties in the commencement are the prejudices on the part of attendants and others, against the abolition of restraint. Attendants who have been brought up in asylums where restraint has been carried out, are usually unable to understand how patients can be managed without, and they will even oppose it, and tell the medical officer that it is impossible ; and if he still persists in his efforts to get the patient out of restraint, they will fraudulently try to convince and show him that it is impossible. They will even in certain cases go so far as to aid and urge the patient to do violence to other patients, or irritate them so as to cause great excitement, and thus carry their

point, and show the medical officer that he is wrong and they right. I have known this to be done, and it is sometimes not difficult to convince medical officers in this way. An attendant who shows the least desire to frustrate any plans of a medical officer should be immediately dismissed ; and nothing short of that will do at the outstart, for one bad attendant will spoil many.

Having restraint apparatus in an asylum has to a certain extent a demoralizing influence upon medical officers as well as attendants. Whatever difficulties may arise, there at once appear before them visions of camisoles, muffs, straps, etc., hanging on pegs ; and it is so very easy to solve the difficulty and save an immense amount of trouble and mental perplexity to the medical officers, that they order the attendant to put on the camisole. This also suits the attendant, for he, to be sure, has a struggle with his patient, but he gets him in the camisole, and then very often thrusts him into a room and is rid of him.

If there is no restraint apparatus the medical officer is obliged to go to his patient and find out what has been the cause of his excitement, and learn all the surrounding circumstances, and from this to find a way of managing the patient.

One of the great difficulties, especially with old attendants who have been brought up in asylums, is, to prevent and make them understand not to interfere with patients unnecessarily ; for instance, take a comparatively quiet ward, as you go through you will find the attendant making the patients all sit down in rows against the wall. And I have even seen one superintendent walk through his ward when all his male patients were sitting in this way ; one of them quietly arose from his seat, when he was at once pushed by the medical officer himself into it again.

This disposition to interfere unnecessarily with patients can be seen in its mildest form by any one as they pass through an asylum, even when accompanied by a medical officer. The patients arise and walk toward you ; observe now the attendant trying to make them go away and sit down ; this is very hard to make attendants believe they must not do. Two motives appear to actuate them in this : 1st, out of deference to the medical officer

who, they think, does not wish them to be disturbed ; or, 2d., because of the evident fear which some people display in passing through the wards of an asylum, and which is due to the common idea that an insane person is like a wild beast.

Take another example, an excited man with chronic or acute insanity. He is in a ward walking up and down excitedly, perhaps talking loudly: the attendant follows him about: he goes into a bedroom ; the attendant pulls him out : he goes to the hall door, perhaps tries the knob, or kicks the door ; attendant rushes after him and pulls him away : he goes to the window ; attendant thinks he is going to try and break out, takes him away from there and tries to make him sit down : each effort of this attendant has been making the patient more and more angry, and at last he strikes the attendant ; he is put in a camisole after a great deal of struggling, and is hereafter looked upon as a bad man. Now all this might have been obviated and the camisole unnecessary if the attendant had only let this man alone ; patients should only be interfered with when they are doing injury to themselves or others, or destroying furniture, etc.

This idea that patients must be followed about and put in restraint apparatus if they manifest the least excitement of manner, etc., so dominates the minds of attendants and others in charge of asylums that it is hard for them to believe that patients can be managed otherwise.

This is shown by a recent experience. The clerk to the Kings County Asylum, Mr. Edward Shannon, recently took eight patients to the asylum at Binghamton. Several of them were cases of chronic mania. One of them was particularly talkative and demonstrative, constantly moving about, talking in loud voice, gesticulating, etc. This man had never been in restraint (he had been in the asylum for a year and a half). These eight men were taken to Binghamton without any restraint apparatus by Mr. Shannon, in company with Dr. Hoyt (the patients were State paupers). On the way, patients from other asylums came on the train, and these patients all wore restraint apparatus, although they were cases of senile dementia and quiet terminal dementia. On ques-

tioning the persons who accompanied these patients, Mr. Shannon was told that they were quiet patients, but they thought it was best to put them in restraint.

Here was a marked contrast—one group of patients noisy, talkative, demonstrative (but harmless), without restraint apparatus ; the other group quiet, feeble, demented, inoffensive, all in restraint.

With ordinary suicidal patients they can be cared for easily without restraint of camisole or crib, and prevented from committing suicide by a faithful and competent night-watch having these patients under observation all night. They are generally quiet, and if in sight will not try to injure themselves ; but it sometimes happens that you meet with a patient who makes continued efforts of all kinds to commit suicide. In the two years that non-restraint has been carried out at the Kings County Asylum one such patient has been met with. She made many suicidal attempts. She would try to suffocate herself in a straw bed ; she would cover her head with her blanket in the presence of nurse, and try to strangle herself with her hands ; she would strike her body on the floor, strike her head against any thing that was hard—wall, floor, bedstead, etc.; tried to pull her tongue out and bite it off. She had advanced phthisis, extensive laceration of cervix, and atresia vaginæ of recent origin, the result of sloughing of vagina at a very recent labor. These are very difficult cases to manage, as it is almost impossible to prevent them from bruising themselves, no matter what means you employ. The camisole would not prevent such a patient from striking her head against the wall, etc.; the crib would be as useless, for they could strike themselves against it. The padded room, with watching of attendant, appears to be the best method of caring for such a patient. Fortunately these very trying cases are not frequently met with.

Seclusion of patients (that is, removing a patient to a room and locking the door so as to retain him there against his wish) is a method which can very easily be abused, unless special care is exercised ; and it is true that it may be made to take the

place of another abuse, namely, restraint apparatus. The seclusion of patients must be kept under the control of the assistant medical officers, supervised by the medical superintendent, and this seclusion should be discontinued just as soon as it is possible to do so. Attempts should be made from time to time to keep the patient out in the hall. In this way you are less likely to keep the patient in seclusion longer than is necessary. In an asylum which is not over-crowded it is much easier to get along without seclusion, or with a very small amount, than in one very much crowded ; for, as a rule, seclusion has to be resorted to for preventing a patient from injuring others.

It is hard to conceive how any one can prefer the use of the camisole to seclusion. If one who has seen them both used asks himself which of these two methods he would rather have applied to himself, I think he would be very apt to prefer a temporary locking up in his own room. Besides, a man who is violent and excited, with a camisole on, can walk up and down the hall among his fellow-patients, and, if disposed, can do them just as much damage as if his hands were free, because he can and will kick them just as much as he pleases. In a room he cannot do this.

It is sometimes said by persons visiting an asylum without restraint apparatus that the patients in this asylum are not the same kind which are found in another asylum where restraint apparatus is used. This is a very great error, and if any one has seen for some length of time both methods of management, he will become convinced of it.

The truth is that the patients do really differ as far as their conduct is concerned, but no farther. They are probably all of the same nationalities and come from the same walks in life, but in the asylum without restraint the patients are quieter and better behaved, simply because they have not been and are not irritated by restraint apparatus. And a patient first admitted to an asylum and put in restraint can soon become a violent patient from this cause alone.

And it is in this way that the asylum with restraint, from its method of management, forms the violent character of its patients;

and it is in this alone that the patients in the two asylums differ. One irritates the patients with restraint, and the other does not.

This is very clearly shown by Dr. McDowall in a review of an article by Dr. Billod, published in the *Journal of Mental Science*, Jan., 1882, page 596. He says Dr. Billod is right when he says that the use of restraint is simply a matter of asylum organization. "Of this I had a crucial example during my visit to Denmark three years ago. On the island of Zealand there are two asylums, and they are managed on entirely different principles. Both are conducted by thoroughly competent men, who devote themselves heart and soul to their work, and who carry out their ideas of right with great consistency. Yet the contrast between the establishments is surprising. At Roskilde Dr. Steinberg carries out the practice of non-restraint, and as one walks through the wards and grounds one is at once reminded of asylums as they are at home."

"Seclusion is rarely used, and restraint almost never. This asylum receives most of its patients from Copenhagen. The cases are largely composed of general paralytics and acute maniacs, due to alcohol.

"An entirely different system of management prevails at Vortenborg, a large asylum for the rest of the island. The management is perfect in its way, but it is thoroughly French. If on admission a patient is excited and destructive, he is secluded and restrained until the excitement disappears.

"I therefore saw a patient in every single room in the asylum, and some had been in these rooms from a few weeks to more than twenty years. It must be stated that the single rooms are much better than those in English asylums,—large airy rooms with windows in the roof. Across these windows it is possible to draw blinds to limit the amount of light admitted. Dr. Fürste found that the amount of light influenced the excitement; the more light the more excitement. There were strait waistcoats in abundance. It must not be imagined that all this restraint and seclusion are employed to save trouble. They are employed on principle as the best method of treatment.

"The staff is large—one attendant to six patients—and the supervision by the superior officers thorough and untiring. Dr. Fürste spends far more time in the wards than any English superintendent I know—on an average ten hours a day; he is evidently popular with his patients, many of whom told me that they are very kindly and considerately treated. In spite of all this I could not help condemning a system which locks up patients for years, and I can never forget the case of an old woman, an amusing chronic maniac, who had not seen the sun or a blade of grass for more than twenty years.

"My Danish experience settled the matter to my mind. Without denying the influence of racial and other difficulties, it is certain that there is no absolute obstacle to the adoption of the non-restraint system."

It has also been claimed now, as in the days of Conolly, that with the abolition of restraint apparatus there was instituted to take its place the use of sedative drugs, and which was styled "chemical restraint." Conolly denied this, and to-day we have the best argument against this fallacy, in the tables in Dr. Wilbur's article showing the relative amount of sedatives used in the asylums with restraint and those without. Dr. Wilbur's tables are here reproduced (see ARCHIVES, vol. vi, p. 271).

TABLE NO. I.—BRITISH ASYLUMS.

NAME OF ASYLM.	No.	Number of patients	Monthly occasions of restraint.	Monthly number restrained.	Monthly occasions of seclusion.	Monthly number secluded.	Average number to whom chloral is daily administ'red.	Av'rage number to whom hyoscymnia or other narcotic is administered to allay excitement.
West Riding Asylum . .	1	1,410	1	1	2	2	31	38
County Asylum, Chester .	2	533	None	None	None	None	None	Morphia used occasionally
Hull Borough Asylum . .	3	163	"	"	"	"	1	2
Montrose Roy'l Lun. Asl.	4	485	"	"	19	1	3	1
Brookwood Asylum, Surrey	5	1,050	"	"	None	None	10	1
East Riding Asylum . .	6	285	"	"	"	"	½	None
Hanwell Asylum . .	7	750	"	"	"	"	None	"
Burntwood Asl., Litchfield	8	600	"	"	"	"	"	"
Royal Edinbur' h Asylum .	9	832	"	"	20	"	1	"
North Riding Asylum . .	10	546	2	1	3	"	7	9
Royal Asylum, Gartnavel .	11	483	None	None	None	None	6	14
Richmond Dist. Asl., Dublin	12	1,013	"	"	3	1	11	2
Dr. Hill's Norfolk Co. Asl. .	13	620	"	"	None	None	"	20
Kent Co. Asylum . . .	14	1,200	"	"	"	"	"	None
Woodilee, near Glasgow .	15	448	"	"	"	"	"	"
		10,419	3	2	47	4	70½	91

TABLE NO. 2.—CANADIAN ASYLUMS.

NAME OF ASYLM.	No.	Number of patients	Monthly occasions of restraint.	Number restrained.	Monthly occasions of seclusion.	Number secluded.	Average number to whom chloral is daily administered.	Av'rage number to whom hyoscymia or other narcotic is administered to allay excitement.
Nova Scotia Hos. for Insane	1	380	117	11	17	5	None	None
Lond. Ont., Asl. for Insane	2	851	61	8	13	2	"	"
Toronto Ont. Asl. for Insane	3	673	10	3	4	5	3	5
Hamilton Ont. Asl. for Ins.	4	537	6	—	4	—	occasional	occasional
Kingston Ont. Asl. for Ins.	5	430	4	—	13	—	—	—
		2,871	198	—	51	—	—	—

TABLE NO. 3.—ASYLUMS IN UNITED STATES.

NAME OF ASYLM.	No.	Number of patients	Monthly occasions of restraint.	Number restrained.	Monthly occasions of seclusion.	Number secluded.	Average number to whom chloral is administered daily.	Av'rage number to whom hyoscymia or other narcotic is administered to allay excitement.
Northern Hospital, Wis.	1	541	48	—	1	1	24	8 daily
Cook County Asylum, Ill.	2	440	480	—	60	—	33	—
Kings County Asy'm, N.Y.	3	868	None	None	—	8	5½	6 a month
Wooster Hospital, Mass.	4	594	69	—	71	—	22	No record
Retreat for Insane, H'tf'd, Ct.	5	121	2	—	2	—	4	1 daily
Willard Asylum, N. Y.	6	1,727	—	6 daily	7	—	27	10 daily
Athens Asylum, Ohio	7	586	None	None	116	58	20	None
Longview Asylum, Ohio	8	661	8	—	11	—	8	1 daily
Dayton Asylum, Ohio	9	591	118	6 daily	309	—	29	—
Northern Asylum, Elgin, Ill.	10	526	483	—	25	—	26	5 daily
Insane Crimin'l Asy'm, N.Y.	11	131	1	1	—	—	1½	5 a month
Middletown, Conn.	12	582	16	3	43	15	21	1 daily
Minn. Hospital for Insane	13	530	67	43	24	17	6	2
Southern Asylum, Anna, Ill.	14	486	350	—	129	—	12	3
Eastern Illinois Asylum	15	175	—	1	22	—	—	3
Homeopathic, N. Y.	16	244	5	3 daily	—	—	None	None
Central Hospital, Illinois	17	641	483	32	2	—	73	48
Western Asylum, Kentucky	18	473	16	—	12	—	9	8
Hudson River Hospital	19	250	—	6	None	—	29	12
State Insane Hospital, Wis.	20	548	2,547	—	13	—	4	10
Danvers, Mass.	21	643	138	—	161	—	5	3
Northampton, Mass.	22	471	292	25	248	26	None	None
State Lunatic Hospital, Harrisburg, Pa.	23	353	3	—	21	—	18	3
Taunton Lunatic Hospital	24	574	14	—	6	—	20	—
N. J. S. Lunatic Asylum	25	583	180	—	150	—	118	None
Newburg, Ohio	26	625	24	—	38	—	47	21
	—	13,967	—	—	1,254	—	561	139

This table is incomplete in some of its columns because the reports upon which it is based were imperfect.

I have reason for believing that since these tables were drawn up by Dr. Wilbur, an improvement has taken place in these asylums.

J. C. S.

Asylums with restraint use as much of sedatives if not more than those having no mechanical restraint. If this were a correct argument, the asylums without restraint should use a great deal more than those with restraint, which they do not.

The use of the words "chemical restraint" for the administration of sedatives to excited patients is incorrect. Within the proper limits which govern the giving of sedatives, and in fact all medicines, it is justifiable, even proper and necessary, to give sedatives to an excited insane person, as to give opium to a person in physical pain or suffering from peritonitis.

Any one who says that it is unjustifiable to give a man sedatives who is suffering from terrifying hallucinations from excessive use of alcohol, for instance (even if they be of temporary effect), while in connection you give him large quantities of food, etc., for the purpose of restoring him to his normal condition, simply talks nonsense. And it must never be forgotten that a great deal of useless argument and criticism are indulged in by the profession on this subject of giving drugs.

The experience at the Kings County Asylum has been, that with the abolition of restraint, there has been a gradual diminution in the amount of sedatives given both by day and night, and to-day it is very small.

It has always been thought that to carry out non-restraint a large number of attendants were required ; this has also been proved to be incorrect by the experience of the Kings County Asylum, where there is one attendant to fifteen patients on the average. And from recent reports of the Commissioners of Lunacy in England and Scotland, it appears that in their asylums there is on the average not more than one attendant to twelve patients.

But one of the most important points in carrying out the system of non-restraint is to find occupation for the patients. This aids very much in keeping them quiet and more contented, tends to turn their attention to a more normal train of thought, and in some cases prevents the rapid approach of complete dementia.

One great difficulty with American asylums, and it appears to be the same to some extent in England, is the want of occupation for men in winter, and the difficulty of getting both sexes out of doors in winter. The want of out-door exercise and recreation causes a restlessness, and makes it much more difficult to get along with them. It is hoped that a solution of this will be found ere long.

It must not be inferred that restraint apparatus is used to great excess in all the asylums in this country. There are certainly a great many where it is carried to its fullest extent (see article by Dr. Woodside in *N. Y. Medical Record*, March 4, 1882); but it is gratifying to learn from gentlemen well acquainted with many asylums that there has been a diminution of the use of restraint apparatus within recent times, and doubtless this will continue following the same course as it did in Great Britain.

## NEW BOOKS AND INSTRUMENTS.

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**A New Freezing Microtome.** By Dr. WILLIAM HAILES, of Albany, N. Y.

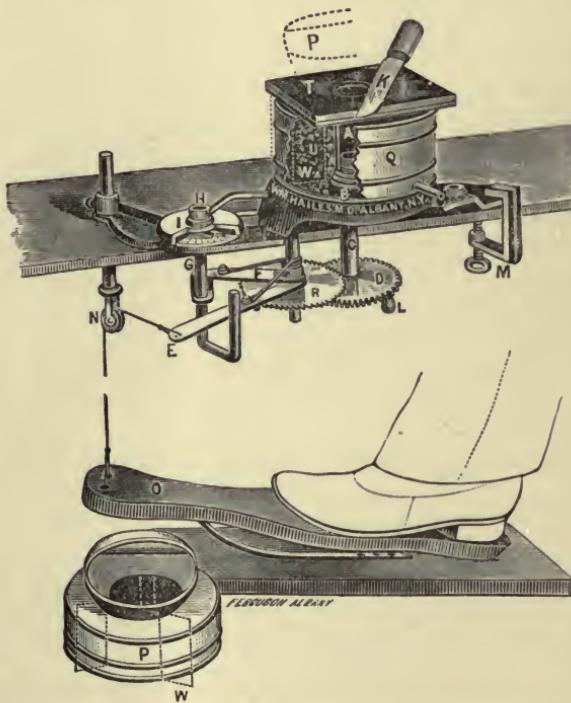
*Directions for using the Freezing Poly-Microtome.*—When the microtome is used for freezing, remove the glass table, cover the ice-jacket with felt or guttapercha to prevent absorption of heat from atmosphere. Oil the screw and plunger to prevent their becoming fixed by the freezing (too much oil interferes with freezing). Screw cylinder into position on bedplate, close the top with a tightly fitting cork to prevent the entrance of ice, etc. Put on the cover and fill the ice-jacket with finely powdered ice and coarse salt through the hopper opening, and stir the contents by rotating the hopper cover. In a few moments the cylinder will be cooled down to freezing point. Remove the hopper lid and cork, fill the cylinder two thirds full with mucilage acaciæ (B. P.), then replace the cork and hopper lid and stir for a few moments.

When a white, frozen film has formed at the periphery, introduce the specimen into the mucilage in the well of the microtome, holding it against the advancing film of ice until it becomes fixed in the desired position. Then pour in a little mucilage so as to cover it completely. Recork and stir, adding ice and salt, as it becomes necessary, until the specimen is *frozen solid*. It is advisable to turn the microtome screw occasionally during the freezing to prevent its becoming too firmly fixed to the sides of the cylinder.

When perfectly frozen, exchange hopper lid for glass table (which has been previously cooled by contact with ice), then cut in the usual way, working the lever alternately with the cutting. The thickness of a section is controlled by the regulator, as shown on the index plate. The thickness most generally preferred is the  $\frac{1}{200}$  inch, the most desirable range being from  $\frac{1}{100}$  of an inch to

<sup>1600</sup>. The temperature at which the best results have been obtained has been when the surrounding atmosphere was about 40° Fah. It works satisfactorily at any season of the year, but requires more experience to work it perfectly in a warm atmosphere, owing to the rapid melting at the surface of the specimen. *All tissues must be frozen solid in order to be cut well.*

Two hundred sections have been successfully cut in a single minute, but a more moderate rate of speed, of about a hundred per minute, is recommended. If during the cutting the tissue should have become partially melted and softened, it must be re-



*Description.*—*A*, small well, tightly corked, fitting on pyramidal bedplate. *B*, pyramidal bedplate containing different sizes. *C*, micrometer screw. *D*, cog-wheel fitting in pinion of ratchet-wheel *R*. *E*, lever actuating micrometer, served by means of a pawl engaging in teeth of ratchet-wheel *R*. *F*, arm, carrying a dog, which prevents back motion. *G*, regulator, limiting throw of the lever, and consequently governing the micrometer screw. *H*, lever nut for fixing regulator. *I*, index with pointer and scale grade from 1-240 to 1-800 of an inch. *K*, knife for cutting sections. *L*, knob to turn micrometer screw direct, when the ratchet-wheel is pushed up out of gear. *T*, table of micrometer with glass top to facilitate cutting. *M*, table clamp. *N*, pulley. *O*, treadle. *P*, hopper-lid with wings, *W*, to replace table *T*, when freezing is to be done.

*Note.*—During freezing the table is unscrewed and kept cool by contact with ice. The freezing is facilitated by rotating the hopper lid occasionally; finely powdered ice and coarse salt are added, as required, through hopper opening. This instrument has stood the test of several years of actual work in the laboratory of our school, and has become an actual necessity; by its aid, in ten minutes, if desired, a thousand sections, from 1-200 to 1-1600 of an inch in thickness, can be readily obtained.

frozen ; this is readily accomplished by disengaging the ratchet-wheel and pinion by pushing it directly upward out of gear. The plunger is made to descend rapidly by turning the micrometer screw directly by knob on cog-wheel. The specimen is then made to descend rapidly. Recork, replace the hopper lid and stir for a few moments. When thoroughly refrozen, the specimen can be returned to the top of the glass table, with its relations unchanged, and the cutting may be continued as before. The sections after remaining in water for 24 hours should be transferred to : Rx Glycerin., aquæ,  $\frac{3}{4}$  iv; acid. carbolicæ, gtt. iiij ( $\frac{1}{4}$  Boil and filter), or kept indefinitely in alcohol. They remain unchanged for years. Perfectly fresh tissues may be cut without any previous preparation, using ordinary mucilage acaciæ (B. P.) to freeze in. If preserved in alcohol, Müller's fluid, etc., it is necessary to wash for 12 hours in running water ; then place in : Rx Sugar (refined or granulated),  $\frac{3}{4}$  ij; aquæ,  $\frac{3}{4}$  j;  $\frac{1}{4}$  l for 24 hours. Then remove to mucilage acaciæ (B. P.) for 48 hours, and cut in frozen mucilage as already described.

The knife employed is an ordinary strong-handled one, ground nearly flat on the under side, and of a very hard temper. It must be strapped frequently.

**The Science and Art of Midwifery.** By WILLIAM THOMPSON LUSK, A.M., M.D. New York : D. Appleton & Co., 1882, p. 587.

This work will be warmly welcomed by students and practitioners. It not only better represents the present state of the science and practice of midwifery than its predecessors, but it is the only obstetric treatise of note that has appeared from an American pen in several years. For these reasons it will undoubtedly replace the English text-books now in so general use in our American colleges.

Much of the new matter contained in its pages has been gathered from the recent obstetric literature of the Germans, who, as the author remarks, have in late years occupied a vantage-ground in this department of medicine.

The general plan of the work is entirely similar to that of the excellent manual of Schroeder. The different departments of the subject are presented substantially under the general heads : physiological anatomy, and the physiology, pathology, and treatment of pregnancy, parturition, and the puerperal state respectively.

A clear and satisfactory account of the anatomy of the generative organs forms the opening chapter. The anatomy of the bony pelvis, however, is treated, where it properly belongs, with the mechanism of labor.

Among the earlier chapters, those on foetal development and the diagnosis of pregnancy may be mentioned as eminently valuable and practical ones.

Dr. Lusk abandons the doctrine of superfoetation; supposed cases of that character he believes to be twin pregnancies in which one foetus has suffered arrest of development.

The mechanism of labor is treated with the thoroughness which its importance demands. Special stress is deservedly laid upon the structure of the pelvic floor and the rôle which it plays in labor.

In the theory adopted from Stephenson, the reader will find a more satisfactory explanation of rotation than in the mechanism generally taught. The theory that the flexion of the head during its passage through the cervix is maintained by the superior pressure of the longer column of amniotic fluid which presses upon the occipital pole of the head, is erroneous hydrostatics. The law of pressure due to mere weight seems to have been confounded with the law of transmitted pressure.

Hohl's method of supporting the presenting part rather than the perineum, is advocated for the preservation of the perineal body. Removal of the forceps before the head passes the vulvo-vaginal ring and rectal expression between the pains are among the measures practised for the same end. Episiotomy is advised when rupture seems otherwise inevitable.

The remarks upon the importance of late ligation of the cord, especially in children born pale and anaemic, should be read by every practitioner of obstetrics.

Of anaesthetics the author says: "The result of my experience has been to make me a warm advocate of their wider employment on the one hand, while proclaiming the necessity for caution on the other." Parturition confers no absolute immunity from the ordinary dangers of chloroform.

The chapter on extra-uterine pregnancy is an excellent *résumé* of the present knowledge of that subject. The chief reliance in treatment in the early months before rupture, is the destruction of the foetal life by means of electricity. A strong Faradic or interrupted galvanic current is passed for about ten minutes directly through the fruit sac without puncture. This operation is repeated

daily, till the tumor begins to shrivel. Prof. Lusk has collected a series of nine cases successfully treated by this method, one in his own practice.

Respecting the use of forceps we note the following statement : " I can only say that with increasing experience my own practice has grown more and more conservative, and my own belief is, that true wisdom requires us to abstain from even trivial operations so long as Nature is able to do her own work without our assistance." The superiority of the Tarnier forceps for all high operations is warmly advocated. This instrument is commended for the delivery of the after-coming head, and for rapid delivery in case of convulsions or similar emergencies. The Simpson instrument is the preferred pattern for general use.

Every skilled operator, we believe, must agree with Prof. Lusk, as to the advantage of applying the blades to the sides of the head rather than to the sides of the pelvis without reference to the position of the head. Transverse positions must of course be an exception to this rule, yet such cases, even, will be best managed by application of the blades in the oblique diameters of the pelvis.

Under the management of pelvic presentations the various methods of breech extraction are treated *in extenso*.

The subject of pelvic distortion occupies three chapters. The relative claims of the different obstetric procedures in deformed pelvis are discussed in a judicial manner.

In contracted pelvis with the head above the brim " there is no rivalry between version and the forceps. Forceps in such a case, the author declares little less dangerous than Cæsarean section. Forceps at the brim, in a flattened pelvis, by causing premature flexion, disturbs the normal mechanism. Statistics are adduced to prove that in all but extreme forms of pelvic contraction, where the other elements of the labor are normal, " Nature will do her own work with the least expense of infant-life and with a relatively small maternal mortality."

We believe that a few years will consign the operation of Cæsarean section to a less conspicuous place in obstetric surgery than the author assigns to it. In any case where Cæsarean section is indicated, certainly the operation of laparo-elytrotomy offers an equal chance to the child and a better one to the mother. We cannot concur in the author's opinion that an imperfectly dilated cervix prohibits laparo-elytrotomy. Artificial dilatation may be practised before the operation, or, as in one of Dr. Skene's cases, after completing the vaginal rent. Indeed, the ease of access to

the cervix after opening the vagina offers every advantage for manual dilatation.

The old practice of introducing ice into the uterine cavity in post-partum hemorrhage seems to us a measure to be tolerated only when other means are not available. Again, why use cold water to arrest the hemorrhage from a lacerated cervix when the hot douche is equally efficient?

The chapter on puerperal fever is a valuable statement of the latest views on this subject. The mortuary records show that in New York City nearly one death in one hundred and twenty-seven is due to this disease, and the importance of a thorough knowledge of the etiology of child-bed fever is justly emphasized. In the light of recent views of surgical inflammations, all puerperal inflammations are regarded as fairly attributable, in the great majority of cases, to the action of septic germs.

While the style of the book is, in the main, lucid, a more systematic analysis of certain portions of the work would bring them more easily within the grasp of the student.

Numerous minor errors occur, necessarily inseparable from the first edition of such a work.

The letter-press is clear and neat. The illustrations are numerous and well executed. Many of the figures are from new sources, several from original drawings.

[c. j.]

### **Nervous Diseases: their Description and Treatment.**

A manual for students and practitioners of medicine. By ALLAN McLANE HAMILTON, M.D., Fellow of the New York Academy of Medicine; one of the attending physicians at the Hospital for Epileptics and Paralytics, Blackwell's Island, New York City; one of the consulting physicians at the Hudson River State Hospital for the Insane, and male and female insane asylums of New York City, etc., etc., etc. Second edition. Revised and enlarged. With seventy-two illustrations. Philadelphia: Henry C. Lea's Son & Co., 1881.

The first edition of this work appeared in 1878. The author states in the preface to the edition: "It has been my object to produce a concise practical book, and should the satisfaction be ever accorded me of knowing that I have made the subjects of diagnosis and treatment of nervous diseases more simple to my readers than I think they now are, I shall be amply rewarded for the task." In the preface to the second edition, he states that it is "enlarged by nearly one hundred pages, and contains many

new illustrations,—in fact this feature of the book has undergone an almost entire change. The enlargement is a matter of necessity, owing to recent advances in our knowledge of neurological medicine."

We will note briefly the principal changes and additions which have been made in the original work. In the introduction the author very properly says : "I would add a word of caution in regard to the error many of us make in too readily accepting and isolating nervous symptoms as distinct which after all may be expressive of some general disorder. It too often happens that simple digestive disturbances,—cholesteræmia, or perhaps uræmic poisoning—give rise to symptoms that are seized upon as the basis of a distinct nervous disease, and the error is not recognized in time to arrest the true mischief."

To the "Scheme to be used in the Examination of Patients," the author has made the following additions : Under the heading, "Motility": "condition of reflex, excitability." Under "Tremor": "whether evoked by jarring the limb, or by tapping tendons or muscles; accompanied or not by pain; associated or not with rigidity of joints when limb is flexed." Under "Sensation": "appreciation of form." Under "Disorders of Organs of Special Sense": "the existence of color-blindness." Under "Speech": "visual and auditory relations." Thus, as in the first edition, this "scheme," while it includes a great number of points, is too defective in its classification to be otherwise than confusing to the student unfamiliar with the subject.

Under the heading, "Instruments Used for the Diagnosis of Nervous Disease," the author has added several pages on cerebral thermometry, referring principally to Dr. L. C. Gray's valuable observations. He has omitted entirely, however, any reference to Dr. R. W. Amidon's laborious experiments pertaining to the subject.

The percussion hammer, for producing the tendon reflex, is added to the list of instruments. We are surprised at the experience of the author with the Leclanché element for permanent batteries; he denounces it as unworthy of recommendation, and states that "it is dirty, inconstant, and rapidly loses power"; while, according to most authorities, just the opposite of these qualities constitute its chief advantages, if it is properly used.

We like his position concerning statical electricity, in reference to which he says : "Beyond its moral effect upon the patient, especially if there be hysteria, I do not believe that it possesses any advantages over the chemical currents."

We are pleased to see that the author has laid stress upon the use of *large* doses of potassium iodide in a variety of nervous affections. In this respect the book stands in decided contrast to most of the foreign works on the subject, in which, as a rule, the dose recommended is ridiculously small, when judged by the experience of American practitioners.

The author has added to the chapter on "cerebral haemorrhage" a *résumé*, in eight pages, of anatomical and physiological data, pertaining to localization theories, and refers also to Brissaud's studies of post-hemiplegic contracture, and the tendon reflexes. Three pages, under different headings, have been introduced on cerebral syphilis.

In the division on "Cerebral Anæmia," additional notes on treatment have been made, principally in reference to the author's own experience in the use of nitrous oxide.

A *résumé* is given, in two pages, of Bastian's and Ball's articles on aphasia; or, as the author prefers to term it, "Asemasia." Charcot's and Ferrier's views on the decussation of the optic tracts are also mentioned. A new chapter on "Diseases of the Cerebellum" has been added, consisting of twelve pages. The subject of acute ascending paralysis, has finally been given a place, though it was omitted in the first edition. A separate division on sclerosis of the columns of Goll has also been added.

The author is incorrect in stating that to Lockhart-Clarke belongs the credit of having discovered the central origin of progressive muscular atrophy, as Luys preceded him in this matter.

The article on pseudo-hypertrophic muscular paralysis has been very much improved by a revision of the contributions of Gowers and others.

The author recedes from his former opinion, that fatty food is contra-indicated in this disease, and adopts the opposite view, that the absence of fatty food is detrimental.

The chapter on diseases of the lateral columns has been increased in size from less than two pages to more than twenty-three, the author having incorporated in it a part of his American Medical Association Prize Essay.

In the chapter on "Posterior Spinal Sclerosis" numerous changes and additions have been made. The description of the characteristic pains of this disease is misleading from the statement that they "dart from the feet up the legs and thighs." It

states also that "every pronounced case invariably presents three marked symptoms. (1) Peculiar pains usually seated in the lower extremities. (2) A simple atrophy of the optic disc. (3) An impairment of the reflex function, usually found in the tendon of the quadriceps, or shown in tardy action of the pupils. These symptoms are constant." Ross, on the contrary, states that atrophy of the optic disc occurs in about 30% of all cases of the disease. It is in this chapter that particular attention is given to the tendon reflexes, which, strange to say, found no place in the former edition, either in regard to the particular disease, or, as far as we have been able to note, in reference to any other. The conditions of the pupils in this disease, to which no reference whatever was made in the first edition (not even excepting myosis), are described in the work before us as follows: "The pupils are sluggish, and sometimes entirely insensible to light. They are as a rule both contracted, though they may be unequal. Jackson, alluding to this state of the pupil, which he calls the Argyle Robertson's symptom, states that he believes it to be due to a loss of reflex activity, and but a link in the chain of disordered functions which in the lower extremities is expressed by the absent tendon-reflex." It is left entirely in the dark which of these various conditions constitutes the symptom in question. As a matter of fact, none of the conditions mentioned correspond to the "Robertson symptom," which consists of immobility of the pupil to light, associated with normal reaction to accommodative movements.

The subject of arthropathies receives more attention than it obtained in the first edition.

The author has the merit of being conservative on the subject of nerve-stretching as a treatment for locomotor ataxia.

Concerning the differential diagnosis of this disease, no reference has been made to the spinal form of multiple sclerosis, a condition which is perhaps more frequently confounded with typical systematic sclerosis of the posterior columns than any other.

To the division on alcoholism, is added a paragraph on "acute absinthism," and a new division on "nicotinism" has been well done. The article on hystero-epilepsy has been enlarged by an abstract, and by cuts from Bourneville and Régnard's work, and also by remarks on the "hysterogenic zones." There are additional remarks on the treatment of exophthalmic goitre, and a few lines on the use of hyoscamine. Vigouroux and Granville's "*percuteur*" for the treatment of neuralgia is described, and an

instrument for the same purpose, constructed by the author. He also introduces a method for the treatment of torticollis by which a descending continuous current is applied to the affected muscle, while the opposite muscle has the positive electrode of the primary inductive coil in contact with it. While it is evident that the object here is to apply galvanism to the affected side and faradism to the opposite sides, he author has not explained his views clearly as to what he expects to accomplish by the definite polar arrangement which he recommends.

We are disappointed in finding in this new book, that the author, who has already published a manual on electro-therapeutics, has nowhere mentioned, when considering electro-diagnosis or treatment, the qualitative changes in reactions of muscles to galvanism, found in a variety of diseased conditions. No reference is made to the "degeneration reaction," the importance of which in its relation to diagnosis and prognosis is so well established.

Many cases have been omitted which appeared in the former edition, and also the list of the formulæ at the end of the volume. This, we believe, is a fair statement of what the new edition presents, and in answering the question: To what extent has the author fulfilled in his second edition the object avowed in the preface to the first? we are compelled to reply, that as far as conciseness and system are concerned there is very little improvement upon the first edition. Nor are the corrections and additions as extensive or complete as the subject and the book demand, or as we expected to find. It is unfortunate that a work, parts of which are so well executed, should be so extremely defective in others. In short, the author has not, apparently, devoted the time or exercised the care in the revision of his work which it required, or of which he is capable. And too many of the alterations in the new edition are but corrections of defects which ought not to have appeared in the first.

[W. R. B.]

**A Practical Treatise on Hernia.** By JOSEPH H. WARREN, M.D. Second and revised edition. Boston: James R. Osgood & Co., 1882.

This is a very good encyclopædia—on a small scale—of the modern operations for hernia, and however poorly Dr. Warren appears as an author, all will give him credit for being a good editor, for in this volume of 428 pages we have papers by Samuel Osborn, Esq., Drs. Greensville Dowell, Henry O. Marcy, and Janney, by Claude Bernard, Mr. Spanton, Prof. Annandale, Dr. H.

Braun, and Dr. B. Codman of the firm of Codman & Shurtleff. We have also liberal extracts from Wood, Allen Burns, Gray, Astley Cooper, Birket, etc., etc. In the prefaces so many men in and out of the profession are thanked for real or fancied services done, that one wonders wherein Dr. Warren's own labors appear. The term litholapaxy was spelled *lithopaxy* in the first edition, and naturally we thought it an error of the proof-reader, but when we find it misspelled in the second edition we naturally infer that the doctor does not know how it should be spelled. Sir Henry Thompson certainly could not have "allowed me to witness his operation for *lithopaxy*." In the volume, too, are papers read in Vermont, and Otsego Co., N. Y., introduced without curtailing them of the stereotyped introductions. And that no original work may escape republication, a communication to the *Boston Med. and Surgical Journal* on the removal of tumors from the breast and nates is introduced to illustrate the natural contractility of arteries. The new chapters in this edition are: I, Causation of Hernia; X, Recent Operations for Hernia, in which occurs Mr. Spanton's paper (27 pages), Prof. Annandale's (3 pages), and Dr. H. Braun's on "Czerny's radical cure" (28 pages); XI, Artificial Anus and Wounds of the Intestine, the brief history of which as given may be interesting to the student of medicine; XIII, Hydrocele and Varicocele, inferior to those given in text-books on surgery; XIV, Observations on Hernia, being annotations and errata by which he can claim for this a new edition and thus save labor; and XV, Résumé and Clinical reports.

In the body of the work are given three very fair plates, reproduced from Bourgery and Blaudin, all inserted into the *second* edition and having no connection whatever, by reference, with the text. As it is, they are placed at convenient distances apart, so that if one should weary reading he can study pictorial anatomy.

The profession at large is indebted to Dr. J. Henry Davenport for a knowledge of the Heaton method of treating hernia by injecting the *canal* with white oak bark. Dr. Davenport edited Heaton's book in 1877. Dr. Warren did not operate at all until July 10, 1879, and did not publish his book until 1880. And yet we are to give Dr. Warren credit for making known to the profession the Heatonian method, or, as he prefers now to call it in his new chapter of annotations, the "Pancoast operation for the cure of hernia by the subcutaneous method."

Inasmuch as "my *improved* operation would seem to be more

acceptable to the better and greater part of the profession than previous operations, if I can judge by the letters of congratulation I receive from distinguished surgeons of this and other countries," it may be well to look into the difference between Heaton's method and the Warren modification. The one used a plain syringe with brad-awl needle, perforated transversely near the extremity; the other uses a spiral needle attached to a syringe, so arranged that the contents can be seen and the contents of the injecting fluid measured, like the barrel of an ordinary hypodermic. The piston works by a spring, and there is at the lower end of the barrel a concave semicircular handle. This is much more cumbersome-looking and more complicated than the Heaton syringe, and practical surgeons in New York, at least, tell us it is inferior to Heaton's. Warren adds alcohol, sulphuric ether, and tinct. veratri viridis to Heaton's formula. Both occasionally get "inflammation, swelling, and abscess."

Dr. Warren would have us believe that he is the more honest of the two; indeed, the care he takes to assert and to re-assert throughout the whole volume that *he is honest*, makes one feel embarrassed in accepting any statements from a man who feels it necessary to go about placarded "I am honest." In chapter VII we are told: "When this operation is attempted upon persons in poor and indifferent health, or of great delicacy, enfeebled by age, or a broken constitution; upon those who have lived lives of intemperance and debauchery, or who are suffering from syphilis or scrofulous affections; upon those living in crowded and unhealthy places, as in the filth and poverty of a great city; upon those in hospitals and public institutions, as almshouses, jails, places of detention, or prisons; or upon poorly-nourished, and anaemic, and upon dispensary patients, the prognosis will be very unfavorable and the chances of success very small and uncertain." That sentence reads like a plagiarism from the common prayer book, and one naturally cries out, "Who, then, Lord, will be saved?"

Let us hope that the next edition will be expurgated of such sentimentalism, will be arranged in a style more suitable, will show less of this haste and more work. There is certainly material enough in the book to make a volume that will do credit to the classic city whence it comes, and will hold out encouragement to the poor as well as to the rich.

[V. P. G.]

Sixth Annual Report of the Managers and Officers  
of the State Asylum for the Insane at Morristown, New  
Jersey. For the year 1881.

Another year brings with it an annual visitor with the above title. It is a neatly printed pamphlet of 32 pages, and purports to supply the citizens of the State with information concerning its conduct and the condition of its family of 586 unfortunate insane persons. First, the managers' report, which covers less than two pages, tells us that they offer to his Excellency, the Governor of the State, the reports of the superintendent, steward, and treasurer, which are appended, and refers the reader to them. It then tells us that an inventory of the *personal property* of the institution shows that its value is \$99,473 14, and that the managers have maintained an "effective inspection of the asylum" by "weekly visits of one or more" of them, and that "memorandums" of such visits "have been made in a book kept for that purpose." They want \$30,000 for improving the grounds, and refer to a gift of instruments of precision, and compliment the superintendent for "suggestions" on the "employment of the insane," which "evince great care in their preparation," etc. We always look to the report of the superintendent, hoping to find something fresh in the way of discovery within the range of cerebral pathology, and if not in that particular line, something encouraging in the line of treatment and its results. The notion of employment "involving muscular exercise and some concentration of attention" being "advantageous as a means of improving the physical, and through it the mental state," is as old as Hippocrates, and as well known to every freshman in a college roster as it is to the learned superintendent of this asylum. But we look over the pages of the report in vain to find a word that sheds any light on pathology, or that points to any plan for preventing or curing insanity, while we are confronted with one humiliating statement, that out of 773 cases that had been under treatment during the past year, there are only 35 "discharged recovered." Also, that out of 1,186 insane persons "received from opening of asylum, August 17, 1876, to November 1, 1881," there have been "discharged recovered, 142."

It is usual to append a table of causes, but the poor ignorant public, who are told so constantly that insanity is on the increase, and that we are all likely to drift in that direction, are left without any notice of what causes lead to insanity, that we may avoid them. The superintendent is, however, not en-

tirely devoid of interest in the unfortunates over whom he is placed. He regards as a subject of "great importance as connected with the facilities of the institution for carrying out successfully its great and benevolent purposes," that the design proposed for laying out carriage drives, approaching the doorways, a central avenue, fifty feet wide, with stone-paved gutters, shade trees and walks, and an ornate gate lodge, etc., should be executed, but knows not whence the money is to come. It does not appear that the fine climate of Morris Plains, the natural woods, and pastoral views, and flowing brooks from the hillside, which furnish such an abundant supply of wholesome water, are sufficiently attractive or invigorating for the semi-conscious multitude who inhabit the palace, without \$30,000 being expended to mar the slopes that nature has made, and decorate (?) with stones and gravel the plains which are better as the gift of heaven than as curious art would make them. But the "Instruments of Precision"! It is a shame to the honored State, which has poured out of its treasury not far from three millions of its money for this institution, that not a scientific step has been taken, not a scientific fact discovered, not a scientific treatment proclaimed since the asylum was organized. The people who give the money have a right to know something about insanity : its causation and symptomatology ; how it can be prevented, and how cured. And now private benevolence has sent a microscope to be used in the line of pathological research. Has it been used ? Has Loring's best ophthalmoscope displayed any lesion in the cerebral structure or its meninges ? and have the other instruments been employed in the field of discovery ? Has any thing been done ? These are questions that intelligent laymen are beginning to press home to those who should answer without the asking. There is beauty without the institution—costly beauty ; but within, the results are not equal to some of our county almshouses, where no beauty is. Public sentiment has been silent too long. The humdrum method of dealing with insanity by trained routinists, who turn neither to the right nor the left, has had its day. The time is at hand when there will be a demand made that cannot be resisted—a demand, first, for scientific knowledge ; next, for the application of such knowledge within the walls of lunatic asylums ; and then for results that shall not make us bow our heads with shame. [J. P.]

## ORIGINAL OBSERVATIONS

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### SARCOMA OF THE CEREBRUM, ILLUSTRATING THE DOCTRINE OF LOCALIZATION.

BY GEORGE L. PEABODY, M.D.,

MEDICAL REGISTRAR AND PATHOLOGIST TO THE NEW YORK HOSPITAL.

The following case is published merely as additional evidence of the truth of the theory of cerebral localization.

J. W., *aet.* 50 ; England ; married ; bookkeeper ; was admitted into the service of Dr. William H. Draper, at the New York Hospital, October 2, 1881.

He says his parents both died suddenly, they having been found dead in bed. They had nine children, of whom one only is dead ; he having died of some liver disease. The others, excepting the patient, are healthy. The patient admits having had gonorrhœa once, but emphatically denies syphilis. He has been a drinker of beer and whiskey. Had erysipelas in childhood, which accounts for certain scars on one of his legs. Has never had rheumatism. Gives no history of renal, pulmonary, or cardiac disease, or of injury. Last winter, after a shower-bath, he experienced a sense of numbness in the left foot, from the great toe to the ankle, which persisted for some time, but to which he paid no particular attention. In the early part of this year he felt somewhat run down in general health, but with no very definite symptoms. For this he travelled. Last June (four months ago), in the early morning, while still in bed, he had, on awaking, a dull pain about the left hip joint, and a sensation as if all were gone below that on the left side. Was able at this time to move the left leg. Soon thereafter there began a spasmodic up-and-down jerking of the whole pelvis ; excruciating pains in the left foot, leg, and thigh ; and

sudden, violent, clonic spasm of the whole left lower extremity (the thigh being suddenly and completely flexed upon the abdomen). The pain seemed then to creep up to the left arm, and to the neck. After this he lost consciousness, and remained unconscious for an hour. On regaining consciousness there was no aphasia, but his tongue was terribly bitten. After the attack he regained completely the power in his limbs. The patient then remained in good health up to two or three weeks ago, at which time, under the same circumstances and surroundings, he had a second attack, which differed from the first only in that he did not lose consciousness and did not bite his tongue. He regained power on the same day, and attended to his business as usual. Six days ago he had a third attack exactly like the second, except that it began by clonic spasms in the left arm. During the next two days he went to his place of business as usual. During the following two days he was only able to drag himself about. After this time, had to give up, being able to get out of bed only with assistance. Loss of muscular power has daily deepened.

There has been no cincture pain, or sphincter trouble; no pain in the head, and no aphasia; also no urinary or alimentary disturbance.

*On admission*, pulse, respiration, and temperature are normal. He is fairly nourished; there is no oedema; no loss of cutaneous sensibility; considerable though not complete loss of power in left upper and lower extremities. Trial by dynamometer shows left hand 0, right hand 80. There are no ocular symptoms, and no deviation of the tongue. Heart, lungs, liver, and spleen are normal.

During the month following his admission to the hospital, he had, at intervals of a few days, marked convulsive movements of the left side of his body and extremities. He was during this time given only bromides of potassium and ammonium, with an occasional small dose of morphine. It was noticed that after each of these spasmodic attacks the patient would lose for several hours the little remaining power of his left extremities. At the beginning of his second month in hospital (November 9th), he was given mixed treatment, which consisted of hydrarg. biniodid., gr.  $\frac{3}{4}$ , and potass. iodid., grs. x. This dose was given three times a day. During the month of November there was no marked change in his condition. The muscular spasms became rather more frequent, and his general nutrition suffered perceptibly.

Early in December his convulsions became more severe and more general, and then, for the first time since he came under observation, they were accompanied by periods of complete unconsciousness, which lasted several hours. At this time, too, he began to complain of intense pain over the right hemicranium. Occasional rigidity of the right half of the body was now noted. On December 17th it was noted that the right half of his body was quite stiff, and that spasmodic contractions of the muscles of the right hand and forearm occurred. The face became distorted and drawn toward the right side, and his tongue deviated toward the right. He now began to lie in a dull, lethargic condition during much of the time, but he could be roused to answer questions about himself. At this time he passed urine and faeces involuntarily. There was no strabismus at any time. From time to time now his temperature became elevated; up to this time, however, never reaching  $101^{\circ}$ .

On December 20th he became comatose, and his temperature rose to  $104.1^{\circ}$  F. From this time his temperature steadily rose until he died, comatose, of heart failure, at 10.30 P.M., on December 21st, with a temperature of  $108.7^{\circ}$  F.

*Autopsy 12 hours after death:*

Body emaciated. Rigor mortis well marked. There is no œdema. There are superficial scars on the external aspect of the left leg; none on the penis, or elsewhere. The following measurements of the limbs were taken:

Circumference of thighs at middle, R., 36.40 cm.; L., 31.40 cm.

Circumference of legs 10 cm. below patella, R., 27. cm.; L., 25.80 cm.

Circumference of arms at middle, R., 22 cm.; L., 17.40 cm.

Circumference of the forearms 8.50 cm. below tip of olecranon, R., 22.20 cm.; L., 18.80 cm.

Rigor mortis is less firm on the left side than on the right. There is a small superficial bedsore over the upper part of the sacrum.

*Brain.*—There is marked flattening of the convolutions generally, which is more marked on the right side. The pia mater is whitened and thickened and firmly attached to the dura mater over the posterior part of the first frontal convolution on the right side. After removing the brain from the calvarium and placing it upon its base, it is noticed that the left hemisphere falls away from the median line, partially collapsing as usual. The right

hemisphere does not collapse at all, its highest point (fissure of Rolando) being on a plane 1.75 cm. higher than the corresponding point of the left. There is a bulging of the anterior  $\frac{2}{3}$  of the right hemisphere inward across the median line. There is distinct fluctuation obtained over the anterior half of the right hemisphere. The brain was then put into chromic acid for hardening, it being merely noted in addition that the arteries at the base are normal.

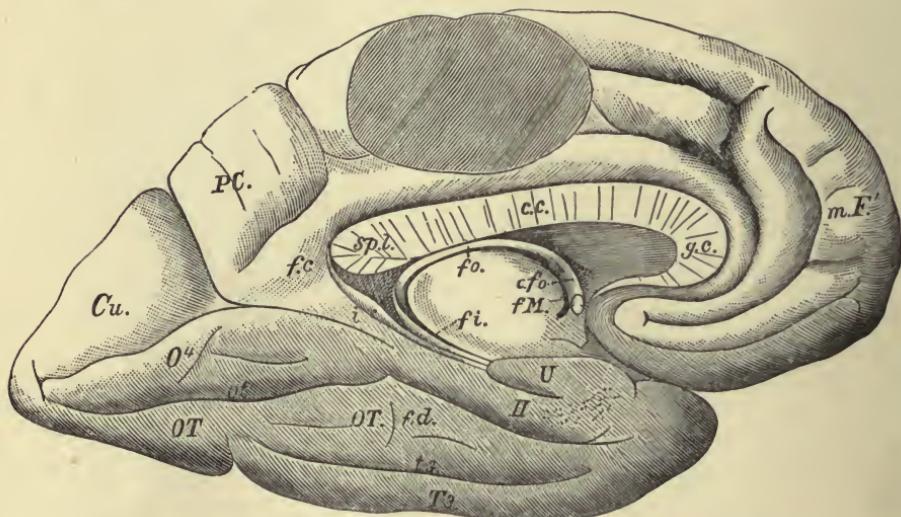


FIG. 1.—View of inner surface of left (should be the right) hemisphere, after Schwalbe. Shaded spot indicates the location of the tumor.

On subsequent examination of the brain a tumor was discovered in the right hemisphere, ovoid in shape, with a long antero-posterior diameter of 5.60 cm. and a short diameter of 3.70 cm. It begins in the white matter of the paracentral lobule just at the terminal portion of the fissure of Rolando, and lies beneath the upper half of the ascending frontal convolution and the posterior part of the first frontal convolution. It has invaded and destroyed much of the gray matter of these two convolutions, but has not reached any of the gray matter elsewhere.<sup>1</sup>

Microscopically the tumor was found to be a sarcoma which contained both round and spindle cells.

The other viscera were substantially normal.

<sup>1</sup> Compare a similar case published by me in *Journal of Mental and Nervous Disease*, 1881, No. 3, case 2, p. 515.—[EDITOR.]

The cuts given show approximately the position of the tumor.

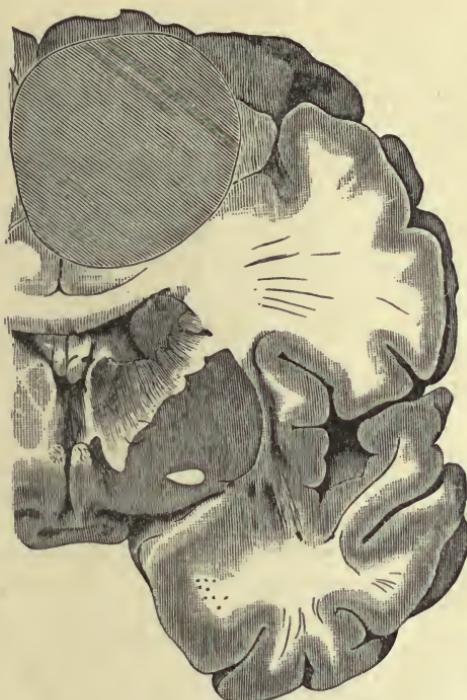


FIG. 2.—Transverse vertical section of right hemisphere, after Fig. No. 4 of Bitot. Shaded spot in upper part of figure indicates the location of the tumor.

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#### URÆMIC DELIRIUM AND COMA AT A VERY EARLY STAGE OF INTERSTITIAL NEPHRITIS.

By WILLIAM OSLER, M.D., M.R.C.P. LOND.,

PROFESSOR OF THE INSTITUTES OF MEDICINE, MC GILL COLLEGE, MONTREAL.

The chief points of interest in this case are: (1) the onset of the symptoms with delirium; (2) the mental worry which preceded the attack—a prominent factor in some instances; (3) the apparently normal state of the kidneys, which gave evidence of changes only on microscopical examination.

J. W., aged 44, a large, powerfully-built man, railway foreman, was admitted into the general hospital, May 6th, with delirium. For past fifteen years had been temperate; prior to this had taken a good deal of alcohol. Up to present attack had enjoyed

good health ; his wife states that she had noticed of late that he got up at night to make water, and he passed rather more than usual. For about a week he had been greatly worried, as a strike had occurred among the men in his department. On the morning of the 4th he had a chill, and felt unwell all day. On the 5th, though still ailing, he went to work, but returned in the afternoon complaining of soreness over the whole body, headache, and chilly feeling. In the evening he became delirious and was noisy and excited all night ; could not be kept in bed, but walked about incessantly talking and directing his men at their work. On the 6th the delirium persisted, and in the evening he was brought to hospital. On admission the temperature was  $100^{\circ}$ , pulse 120. He passed a very restless night, and was with difficulty kept in bed. In the morning (7th) he was quieter ; temperature  $100^{\circ}$ . When seen at the mid-day visit was quieter, but did not answer questions intelligently. Face flushed, venules on cheeks and nose dilated. Pupils slightly contracted, react to light ; ophthalmoscopic examination of eyes negative. No special symptoms in chest or abdomen. No dropsy, nor œdema of ankles. Heart's impulse not forcible ; apex in normal position, but difficult to feel ; pulse full in volume, tension plus ; radials not stiff. Urine was passed in bed ; that drawn off with catheter, high colored, sp. gr. 1039, highly albuminous, with numerous finely granular casts, many of unusual length. Toward the afternoon he slept. In the evening was very torpid ; did not know his wife. Temperature normal. On the 8th, after a quiet night, he was very drowsy, roused with difficulty ; pulse 120, temperature  $99\frac{1}{2}^{\circ}$ . Pupils of medium size, react slowly. Passed 24 oz. of urine ; same characters as before noted. Toward the evening he became deeply comatose ; the respirations increased. He was bled to 20 oz., with the effect of reducing the rapidity of pulse and respirations. Pupils dilated ; temperature rose to  $103\frac{1}{2}^{\circ}$ . The coma increased, and death took place at 3 A. M. on the 9th. The treatment consisted in bromides and chloral in the early stage ; purgatives, pilocarpin, and vapor baths.

*Autopsy.—Brain* : Arachnoid turbid at base and over the sulci ; much serosity about the membranes, which stripped off very easily. Several slight ecchymoses in gray matter of right hemisphere ; one at top of ascending frontal gyrus was the size of a small pea. Ventricles contain a moderate quantity of fluid ; walls not softened. On section, substance of the organ not specially moist. The arteries at the base not atheromatous. *Heart*

weighed 382 grammes; valves healthy, muscle substance of good color; walls of left ventricle measured from 15-18 mm.; chamber, 8.5 cm. from apex to aortic ring. Aorta presented a few scattered patches of atheroma. Nothing of special note in *lungs, spleen, stomach, or intestines*. *Kidneys*: right, 190 grammes; left, 175. Capsules detach readily and leave smooth surfaces; nowhere granular. Organs cut with moderate firmness; cortices not diminished; medullary rays very distinct; intervening vascular regions with the tufts injected. Arteries at bases of pyramids not unusually prominent. Pyramids look normal. Renal arteries not atheromatous. Altogether, the *macroscopic* appearance of the organs did not appear to substantiate the diagnosis of uræmia which had been made. Beyond a slight increase in firmness the glands certainly did not present appearances which would have attracted further attention had not the symptoms demanded it.

On microscopical examination the only striking change was in the Malpighian tufts, a number of which were found atrophied and surrounded by an increased growth of fibrous tissue. In some, where the process was far advanced, the tuft was converted into a small homogeneous mass, without nuclei; in others, portions of the tuft appeared normal. There did not appear to be any special proliferation of epithelial elements within the capsule, but there was a very general thickening of the delicate zone of fibrous tissue about even healthy-looking ones. In the neighborhood of several atrophied tufts there was a small-celled or nuclear growth separating the tubules. The small arteries presented decided hypertrophy of the muscle elements, particularly in the circular coat; no hyaline degeneration of the intima. The epithelium was everywhere healthy-looking, distinctly granular, but not swollen; and there were no collections of epithelial *débris* observed in any of the tubules. Except in the vicinity of the atrophied tufts no increase in the intertubular connective tissue was noticed. In the pyramidal portion some of the tubules presented finely granular casts.

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#### A REPORT OF ANATOMICAL ANOMALIES.

By A. H. P. LEUF, M.D.,  
OF BROOKLYN, N. Y.

It is my purpose to present, as briefly as possible, some anatomical anomalies, that I have observed in the dissecting-room of the Long Island College Hospital from Oct. 1878 to Feb. 1881. I have arranged these facts in the order adopted by Gray in his anatomy.

BONES.—*Atlas.* In one specimen the vertebral groove on the right side was converted into a canal. In another was seen nature's unsuccessful attempt to make a similar change on both sides, the centre of the superior arch, however, being defective. On both sides of this bone, between the incomplete vertebral canal and the transverse process, was situated an incomplete foramen, 3 centimetres in diameter, and probably for the passage of the occipitalis major nerve. Henle says: "In one instance the *nervus occipitalis major* passed through a bony canal behind the transverse process."<sup>1</sup>

Another subject had its left vertebral groove transformed into a canal, and the spines of the 3d, 4th, 5th, and 6th cervical vertebrae were remarkably bifid, each ending in a long and a short process. The average length of the long one was 1.5 cent., and that of the other .5 cent. The long spines of the 3d and 5th were on the right side, and those of the 4th and 6th on the left side of the median line. A neck containing such spines would be very apt to mislead one in an examination, and might prove the cause of an unfavorable prognosis on the part of the physician, and considerable annoyance and unnecessary restraint to the patient.

*Sacrum.*—The posterior arch of the first segment appeared as though its right half had been tilted downward and inward. There were two very distinct spinous processes, although the posterior arch was complete; the left one being longer than its fellow, which it overlapped, and cylindrical in form, while the right was flat, broad, and quadrilateral. The right superior articular surface looked downward, while the left did not.<sup>2</sup> The lower segment of the sacrum was firmly united to the upper piece of the coccyx, as were also the cornua of both bones.

A right *temporal* had a large open cleft in place of the Glaserian fissure, being 2 cent. long and 1 cent. broad at its widest or middle portion, and 5 cent. at its narrowest or external part. It seemed to be due to an arrest of development. Henle mentions this occurrence.<sup>3</sup>

<sup>1</sup>"Einmal verließ der N. occipitalis maj. durch einen knöchernen Canal hinter dem Querfortsatz." Henle: Dritte Auflage Knochenlehre, Erster Band, Seite 51.

<sup>2</sup>I failed to find any mention of this anomaly of the sacrum.

<sup>3</sup>"Oft ist die vordere Wand des Gehörganges in einer kleineren oder grösseren Ausdehnung durchbrochen entweder in Folge mangelhafter Verknöcherung oder durch Abnutzung. Cassebohm: Tract. de aure humana, Hal. 1734, p. 28, Taf. i, fig. 2 r. Dieterich: a. a. O. S. 10, fig. 1 bb. Hyrtle: Spontane Dehisz., S. 6. Retzius: Schmidt's Jahrb., 1859, Hft. 11, S. 153." Henle: Drit. Aufl., Erst. Band., Knochenlehre, S. 156.

The left *ulna* of one subject had the olecranon process replaced by a sesamoid bone equalling it in size, with no other muscular attachment than the triceps. The same subject also presented a *fibula*, of which the shaft of the bone bent inward so as to touch the tibia. This bone was not deficient in calcium salts and all the other bones were well formed. The subject was a male aged about 45 years, and there was no evidence of fracture in the bent bone.

**MUSCLES.**—*Digastricus Accessorius*. Two muscular slips, each 3 cent. long and fusiform in shape, which arose from the fascia covering the hyoid bone at the attachment of the digastric. From thence both muscles passed upward and inward, half way to the chin, where they decussated to the extent of 1.5 cent., the fibres of the right side being superficial.

*Tensor Fasciæ Thoracis*.—This was a continuation of the tendon of the sterno-cleido-mastoid downward for about 2.5 cent., where it expanded into a flat muscle, passing for insertion to the fascia over the 5th, 6th, and 7th costal cartilages. The muscle was about 10 cent. long, and its lower, widest part 3 cent. broad. This specimen was bilateral. I have also seen four unilateral specimens almost identical with the above. Two were situated on the right side and two on the left. Henle describes this muscle under the name "musculus sternalis," and remarks:—"Hallett found it once in every 15 cases. Gruber in 100 bodies 5 times; 3 times bilateral and 2 times unilateral. Turner in 650 bodies 21 times; 7 times unilateral, 9 times on both sides and 5 times oblique, from one side to the other. Wood in 175 bodies 7 times, and of these only one was bilateral."

*Costo-Coracoideus*.—This muscle arose from the upper border of the sternal end of the fifth rib to the aponeurosis of the short head of the biceps, 3 cent. below the coracoid process (left side).

*Chondro-Coracoideus* (left side).—A muscular slip as thick as an ordinary slate pencil, passing along the lower border of the pectoralis major. It arose from the aponeurosis of the hyposternal notch and from the seventh costal cartilage, and was inserted into the conjoined tendon of the biceps and coraco-brachialis, 2.5 cent. below the coracoid process.

*Pectoralis Minor*.—This muscle had a very distinct origin from the 2d, 3d, and 4th ribs, and was so on both sides of the same subject. The coracoid attachment was normal.

*Tendo Accessorius Subclavii*. The subclavius arose normally

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<sup>1</sup> Henle: Dritte Aufl., Erst. Band., Muskellehre, S. 98, 99.

from the first rib and had a small narrow attachment to the clavicle at the middle of its under surface, where it became tendinous and passed downward and outward, over the axillary artery, for insertion into the coracoid process. Henle refers to Haller (de corp. hum. fabr. vi, 77) while mentioning "an accessory tendon to the coracoid process" from the subclavius.

*Musculus Basis Axillæ* (left side).—This consisted of a muscular slip passing from the upper border of the latissimus dorsi forward across the axilla, in front of the axillary vessels, to be inserted into the under surface of the pectoralis major near its insertion. I noticed a similar slip, also of the left side, in a fœtus at term, which arose from the upper part of the anterior border of the latissimus dorsi, and crossed the base of the axilla to the middle of the lower border of the pectoralis major.

Gray, in his work on anatomy, cautions surgeons to bear this in mind when operating in the axillary space, as it might otherwise mislead them.

*Triceps Flexor Cubiti*.—The third head arose from the internal condyloid ridge, between the insertion of the coraco-brachialis and a point about 2 cent. above the internal condyle.

*Extensor Carpi Radialis Brevior*.—One half of this muscle was derived from the belly of the extensor carpi radialis longior. This head is called by Wood *extensor carpi intermedium*.

*Musculus Flexor Communicans*.—This was a musculo-tendinous slip from the middle of the belly of the flexor sublimis digitorum to the annularis tendon of the flexor profundus. This slip was 1.25 cent. in length, and gave origin to the second lumbrical muscles, counting from the ulnar side.

*Flexor Pollicis Accessorius*.—I have, on eight different subjects, seen a small muscular slip, about 7 cent. long and 15 mm. in its largest diameter. It was fusiform in shape, and had a tendinous origin from the under surface of the belly of the flexor sublimis digitorum, about 2 cent. below its coronoid attachment, and was inserted into the flexor longus pollicis where the muscular belly became tendinous. This slip is not identical with that head of the flexor longus pollicis which is described by Dr. J. F. Walsh, in the *Annals of the Anat. and Surg. Soc.*, vol. ii, pp. 458-9.

*Extensor Ossis Metacarpi Pollicis Accessorius*.—This muscle arose from the extensor carpi radialis longior, midway between the internal condyle and insertion of the pronator teres. It became tendinous at the insertion of the pronator, like the muscle

from which it was derived, and was inserted into the base of the first metacarpal bone, above the extensor ossis metacarpi pollicis.

*Quadratus Femoris Secundus.*—A quadrilateral muscle, about 8 cent. long and 4 cent. wide, which extended from the tuberosity of the ischium to the back of the femur, below the linea quadrati. This muscle was separated from the quadratus femoris proprius above, and the adductor magnus below, by an interval of fully 1 cent., which was filled with loose connective tissue.<sup>1</sup>

**ARTERIES.**—*Left Common Carotid.* In two instances this vessel arose from the *innominate*, and consequently the *aortic arch* was abnormal in giving rise to only two vessels, *i. e.*, *innominate* and *left subclavian*.

*Right Subclavian.*—In one instance this vessel was without a superior intercostal branch, but it gave off two others that were abnormal, 1 cent. external to the thyroid axis. They passed upward to the deep structures of the neck. They were 12 mm. apart at their origin, and measured 3 mm. in diameter.

I observed a left subclavian without a thyroid axis in a foetus at birth. The vertebral was normal. The second branch ran upward and outward, between the anterior and middle scaleni muscles, to pass in front of the seventh cervical nerve, .5 cent. external to the transverse process of the seventh cervical vertebra. From this point it passed behind the fifth and sixth cervical nerves to reach the levator anguli scapulæ, in which it terminated. The *transversalis colli* passed between the two primary trunks of the brachial plexus. The *suprascapular* passed through the lower primary trunk of the same, causing it to split in its passage through it.

*Right posterior temporal* passed behind the ear and under the *retrahens aurem* muscle. The anterior temporal was normal.

*Right Axillary.*—This vessel gave off four large muscular branches and two *thoracicae alares*. A common trunk was given off at the axillo-brachial junction, which divided into superior and inferior profunda. The *thoracica longa* of the same side gave origin to four large *thoracicae alares*, and in the same subject and on the same side the *anterior circumflex* and *superior profunda* arose by a common trunk from the axillo-brachial junction. I

<sup>1</sup> I have sought in vain for a mention of this anomaly in any of the books. This muscle seems to be anomalous in only one respect, according to the books, *i. e.*, in its occasional absence.

failed to see a *superficialis volæ* in the same limb, but instead noticed a branch of the radial, which passed around the back of the first metacarpal bone and terminated in the adductor pollicis. This branch was about 3 mm. in diameter.

The left arm of one subject presented the following: *Posterior circumflex and superior profunda*, by a common trunk, from the beginning of the brachial. *Anterior circumflex and inferior profunda*, by a common trunk, from the brachial, 2.5 cent. below the preceding. The *anastomotica magna* arose from the *inferior profunda*.

The *brachial* divided 1.25 cent. below the internal condyle and immediately above the bicipital fascia. The *radial* passed under the fascia, and its upper 2 cent. was overlapped by the *pronator teres*, and was subcutaneous below this point. The *ulnar* was quite superficial. It was covered anteriorly by the *palmaris longus*, 5 cent. below the internal condyle. It descended to the inner and back part of the forearm, while it lay between the *flexor profundus digitorum* and *flexor carpi ulnaris*, slightly overlapped by the former, from 2 cent. below the middle of the forearm to within 2.5 cent. of the pisiform bone.

*Anterior Interosseous*.—This was not as deep-seated as usual, as it was covered by only three muscles, *i. e.*, radial head of *flexor sublimis*, the *pronator teres*, and tendon of the *flexor carpi radialis*. It was almost as large as the *ulnar*. Opposite the wrist it divided into two palmar branches, each of which divided into two digital branches opposite the metacarpo-phalangeal articulations. The two branches on the *ulnar* side supplied the contiguous sides of the *medius* and *annularis*, and the other two those of the *medius* and *index*.<sup>1</sup> The *ulnar* supplied one and a half on its side, and the *superficialis volæ* was very large, and divided into *princeps pollicis* and *radialis indicis*.

The *median*, in one instance, arose from the brachial, 1.5 cent. above the bifurcation, and accompanied the median nerve. It passed downward between the *flexor longus pollicis* and *flexor profundus digitorum*, and behind the *flexor sublimis digitorum*. This vessel was enlarged to the size of the *radial*. In its passage under the annular ligament of the wrist it hugged the trapezium, and in the palm formed the superficial arch. It sent an independent slip to accompany a digital branch of the median nerve to the radial side of the ring finger.

In another case the median artery was also enlarged, and sup-

<sup>1</sup> I could find no mention of this anomaly.

plied three and a half fingers on the radial side, and the ulnar one and a half on its side. The radial met the deep branch of the ulnar to form the deep palmar arch.

The *aorta* referred to above, in relation to the anomalous origin of the left common carotid from the innominate, was also minus the right eighth intercostal artery.

The *cæliac axis* and *superior mesenteric* arose from the same opening in this vessel ; or rather, the latter arose from the former. The two *renals* were given off only 2 mm. lower down. The left *suprarenal* arose opposite the *cæliac axis*, and the right came from the *renal*.

In another subject the aorta was very much enlarged above, measuring 6 cent. in diameter at the arch, and gradually tapering from this point down to the bifurcation, where the calibre was normal. At its upper part this vessel sagged down to the extreme left of the spinal column ; at the diaphragm it was in front and lower down to the right of the vertebræ, when it again turned to the left and bifurcated opposite the middle of the fourth lumbar vertebra in the median line.

The *common* and *external iliacs* were related quite unusually in this subject. These vessels, exactly the same on both sides, instead of following the inner border of the psoas magnus, dipped down into the pelvis, so that their lowest points rested on the ischiadic spines. The appearance of the vessels was like a short loop of stiff rope. The relations of both to Poupart's ligaments, as they passed under them, were normal. This anomaly should be borne in mind by any one who attempts to ligate this vessel, as otherwise the operator might experience considerable annoyance, and the patient much danger.<sup>1</sup>

Both *obturator arteries* of this subject arose from the inner side of the femoral artery, 1 cent. below Poupart's ligament ; thence going forward along the inner side of the vein passed through the femoral ring, hugging the edge of Gimbernat's ligament. After leaving the ring both vessels pursued their normal course.

In another subject both obturator arteries arose from immediately underneath the ligament of Poupart, so that it was difficult to tell whether they came from the external iliac or femoral. Both

<sup>1</sup> Luschka noticed a similar anomaly, but less in degree. W. Krause, while speaking of the iliacs, says : " Sie bildet am oberen Rande der Incisura ischiadica major eine nach abwärts convexe Schlinge, aus welcher die Aeste der fehlenden A. hypogastrica direct entspringen (Luschka)." In other words, "It forms a downward convex loop on the upper border of the great sciatic notch, from which the branches of the missing hypogastric artery directly arise."

vessels passed downward from their origin, forming a loop about 1 cent. long. On both sides this loop and its returning branch were situated between the femoral artery and vein, and passed through the ring, hugging the pectineal ridge behind the vein, and close to the artery. Thereafter the course and relations of both vessels were normal.<sup>1</sup>

The right obturator artery of another subject arose from the femoral at its beginning, and entered the pelvis at the inner side of the ring. The obturator of the left side differed from its fellow only in origin, which was from the lower end of the external iliac.

I have also seen several unilateral anomalies of this nature, and I distinctly recollect that they were not different from those above, and occurred on both sides.

*Accessory External Circumflex* (right side).—This vessel arose from the *superficial femoral* 3 cent. below the origin of the profunda, and was .4 cent. in diameter. It supplied the quadriceps extensor.

In one instance the left *profunda femoris* was without internal or external circumflex, but 3 cent. below its origin this vessel gave off a branch 1.5 cent. long, dividing into two ascending branches to the iliacus and psoas muscles, and a large descending branch to the *vastus externus*; thence continuing outward to the *tensor vaginæ femoris*, 2 cent. lower down, the profunda sent off another branch, which ran downward and outward, and divided into anterior and external; the former going to the middle of the *rectus femoris*, and the latter to the middle of the *vastus externus*.

**NERVES.**—The *descendens noni* was found within the carotid sheath, and in front of and between the vessels. The *thyrohyoid* branch arose from it, opposite the middle of the thyroid cartilage, and was compelled to pierce the sheath to reach its destination.

The left *recurrent laryngeal* arose from the pneumogastric opposite the transverse process of the third cervical vertebra, instead of opposite the left subclavian artery. Of this I could find no mention.

The right *median* was found 2.5 cent. below the beginning of the brachial artery, and the *musculo-cutaneous* of the same limb failed

<sup>1</sup> Of this anomaly I have also failed to find any mention. It strikes me that the occurrence of the loop must be very rare. Dr. W. Krause (*loc. cit.*) says that the obturator, when arising from the femoral, always passes through the femoral ring to the inner ("medial") side of the femoral vein.

to pierce the coraco-brachialis muscle, and passed across the middle of the median cephalic vein, in front instead of behind. Hence, care should be taken in the operation of phlebotomy, so as not to cut this nerve if it happen to bear this abnormal relation to the vein. I was unable to find any account of these anomalies of the median or musculo-cutaneous.

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# ARCHIVES OF MEDICINE.

## Original Articles.

### THE CARCINOMATOUS METAMORPHOSIS.\*

By A. W. JOHNSTONE, M.D.,  
DANVILLE, KY.

In the winter of 1880-81, while working in Dr. Heitzmann's laboratory in New York, I examined four lymph glands, three of which showed that there are several stages in the formation of cancer.

The first came from a gentleman 48 years old, on whose prepuce a few small nodules had grown.

When his surgeon heard that they were cancerous he amputated the penis, but six months afterward, two lymph glands, one the size of a hazel-nut and the other as large as a pea, were found in his right groin.

Both were extirpated, and although two years had then elapsed, the patient was still perfectly well.

The second gland came from a male inmate of Charity Hospital, N. Y., aged 42. He had a cancer of the throat that bled so freely as to necessitate the ligation of the right carotid. This was followed by excision of the tumor and the removal of an enlarged lymph gland from the posterior maxillary region. The man died a few days after the operation, and at the autopsy small abscesses were found in the lungs and some yellowish nodules in the liver and

\* Read before the Kentucky State Medical Society, April 6, 1882.

kidneys, which the microscope showed to be secondary cancer in its earliest stage of development.

A gentleman of over 50 years furnished the 3d specimen. About a year before he was operated on for cancer of the skin on the left leg. Shortly afterward a number of new tumors arose and the lymphatics of the groin began to swell. These new growths as well as the lymphatics were removed, and parts of them brought to the laboratory. Case 4th.<sup>17</sup> A woman of unknown age was operated on for cancer of the breast in the German Hospital of New York City in 1875. A few of her indurated lymph glands were taken from the axilla, and since then had remained in Dr. H.'s possession.

The last specimen on the study of which this paper is based, I removed, by a partial Lisfranc amputation, from the foot of a lady living in the country, about four miles south of Parksville, Ky. The microscope showed it to be a rapidly growing carcinoma.

The first three specimens showed all the stages of invasion, but the fourth contained nothing but the fully formed cancer tissue.

The transmission of cancer from a primary focus to the adjacent lymph glands is probably done by a transportation of the epithelia of the cancer through the lymph channels to the ganglion. This we know is sometimes done, for in case No. 1 we saw a few epithelia scattered among the lymph corpuscles of the cortical substance, their size and shape easily distinguishing them from all their surroundings.

Of course this will not justify one in denying that the fluid portion of the lymph coming from cancer, the so-called cancer juice, does not transmit the infection. We are sure, however, that cancer epithelia are carried and lodged in the lymphatic glands, but I am equally certain that we cannot yet explain why they or the juice can transform the structure of a normal tissue into that of cancer.

We have a great deal to learn yet before we can understand what gives the power of infection to the elements of cancer and sarcoma.

As I have already intimated, in the first three specimens I could trace the changes leading to the formation of cancer tissue. The first stage that we found was in that part of the gland where no natural fibrous trabeculæ separated the healthy from the diseased tissue. This consisted in a gradual melting down or running together of the lymph corpuscles, and thus forming large multi-nuclear protoplasmic masses, the so-called myeloplates. I have never seen these formations in a healthy adenoid tissue; once, however, I did see a few small ones in a hypertrophied tonsil.

There is no doubt in my mind that they spring from the confluence of the lymph corpuscles in all their different stages of development, as well as from the mucous threads that are their matrix.

This is not the first time that such a confluence of protoplasmic bodies has been proved to occur, for some time ago Ziegler sealed two small pieces of glass partly together and put them under the skins of rabbits. Shortly afterward he found that they contained these myeloplates, the only possible source of which was the migratory corpuscles.

In the lymph follicle the corpuscles are connected to each other by delicate offshoots of living matter, which pierce the separating layer of liquid, so that we can easily understand that all that is necessary to the formation of a myeloplate is the fusion of their jelly-like inter-trabecular substance.

Within one of these large lumps of protoplasm a number of nuclei fade and are transformed into a uniform reticulum of living matter, and thus a formation originates which resembles the myeloplates that are seen wherever bone is about to be made, or wherever it is reduced in the processes of growth and of inflammation.

Where bone is forming they mean that the territories of bone tissue are first laid down in the shape of a myeloplax, but where it is being reduced they mean that the territories, by the liquefaction of their basis substance, are brought back to their original protoplasmic state.

As I have already said, the process of confluence of formerly separated corpuscles is splendidly shown in the earliest stages of a growing cancer. In the invasion of a lymph gland its central portion is generally first involved. Frequently we found an inter-follicular string completely transformed into a continuous protoplasmic mass, but still retaining its original shape. These masses of protoplasm are supplied at regular intervals with large globular or oblong nuclei, which it is highly probable are newly formed and have very little to do with the original nuclei of the lymph corpuscles. It has been shown that the myxomatous reticulum holds a delicate network of living matter, which after the liquefaction of the basis substance held in its meshes, can reproduce protoplasm. This is most probably the process through which the fibrous framework is merged into the same mass with the corpuscles. These masses are coarsely granular, which means that they are freely supplied with living matter at the points of intersection of its network.

The next stage ensues through the appearance of the cement substance in the shape of straight, light lines arising first in the midst of the protoplasm between the nuclei. Under the microscope we see but one projection of the cement substance, but, in fact, it must be considered as a cloak enclosing polyhedral bodies, which are the epithelia. At first the cement lines are scarce, and in many places traversed by broad bridges of protoplasm. Later on it assumes a regular polyhedral shape, though it is always pierced by delicate spokes of living matter, which are the

inosculations of the reticula of living matter contained by the neighboring epithelia. These threads are the prickles of Max Schultze. The next feature is the formation of a frame of connective tissue, which divides the large protoplasmic mass into small alveoli, the cancer nests. The first trace of this formation is the appearance of delicate nucleated spindles, which by being split up into very minute, slender spindles, build up the fibrous basis substance. Its ramifying blood-vessels are formed at the same time.

Not infrequently the cancer nests in the midst of lymph glands exhibit concentric onion-like layers of epithelia, which in all probability are the result of pressure from the contraction of the surrounding connective tissue. In the centre of a nest we often see epithelia undergoing fatty degeneration. Sometimes it has gone to such an extent that a fat-plug is produced, the so-called cancer pearl, which is surrounded by flattened out horny epithelia. I found this concentric arrangement in the first three cases, but the fourth exhibited a fibrous frame enclosing irregular alveoli filled with large granular epithelia, but without any regularity of position. This is generally known as medullary cancer, the other as epithelioma. The essential points in this study of the lymph glands are that their invasion by cancer shows itself first by the melting together of their components, and by this means forming large protoplasmic masses. These, in turn, by the formation of the cement substance, split up into polyhedral epithelia, which in groups become ensheathed by vascularized connective tissue, and thus give rise to the cancer nests.

The study of the fifth case, which I have already said was that of a rapidly growing primary cancer of the foot, drove me to the conclusion that almost exactly the same state of things is going on in cancer wherever it is found. For along the edge of the fully formed carcinomatous tissue we

saw other tissues that showed all the changes that I have just described. I was also convinced by this study that the infiltration of round corpuscles that is always found surrounding a carcinoma is really a part of the metamorphosis, and not an inflammation caused by the irritation of the growth, as has been taught by some. They are exactly the same thing histologically as the lymph corpuscle, and it is by their fusion that the myeloplaxes are formed. Thus, I think we are warranted in saying that they are but a step of the fixed tissue corpuscles in their retrograde metamorphosis to the foetal tissue from which the fully grown cancer nests spring.

For my clinical reader I have but one deduction from all this histological work, and that is, in his operations on carcinoma, to make a clean sweep of all tissues that seem infiltrated in the slightest degree. For if he removes all the completed cancer tissue and leaves only a small portion of this infiltration, he has left the most dangerous part of the whole growth, that which is still progressing.

AN EXPERIMENT IN CARDIO-SPHYGMOGRAPHY ;  
IN WHICH THE HEART, CAROTID, FEMORAL,  
RADIAL, AND POSTERIOR TIBIAL ARTERIES,  
AND THE CORRESPONDING TIME, WERE TRACED  
SIMULTANEOUSLY ; WITH REPRODUCTION OF  
THE INSCRIPTIONS OBTAINED.

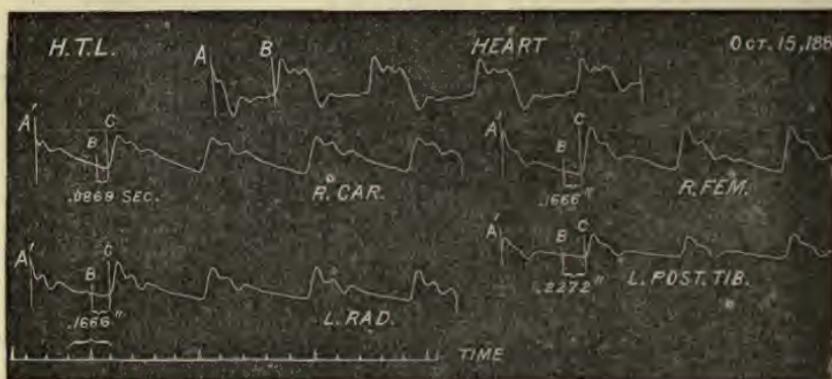
BY A. T. KEYT, M.D.,  
CINCINNATI, OHIO.

THE accompanying illustration is a *fac-simile* of the surface of a glass slide on which were traced the pulsations of the heart and the corresponding pulsations of the carotid, femoral, radial, and posterior tibial arteries, with the time in fifths of seconds. The traces were taken simultaneously by means of five transmission sphygmographs and a chronograph arranged to write upon the same surface. The sphygmographs were sensitive and true and in every respect uniform with each other, and the chronograph was tested for correct time. The subject was a healthy man aged twenty-eight years, placed on his left side to facilitate the taking of the cardiac trace. The pulse-bases were held in their respective positions by the operator and requisite number of skilled assistants. In this manner the experiment whose results are here reproduced, and many similar ones, were made.

In further explanation : The lines A, A', were made by the writing levers with the slide at rest, and are therefore

synchronous signals showing the exact point of evolution of each pulsation at the instant indicated. The lines B and C are artificial, but drawn with care. The first, B, is parallel to A, and cuts the basal or beginning point of the cardiac pulsation ; the second, C, is parallel to A', and cuts the basal or beginning point of the arterial pulsation. The space B-C on each pulse line shows the difference in distance between AB and A'C, and, measured on the chronogram, expresses the delay of each pulse on the heart.

Manifestly the distinctive feature of this species of representation is that the same pulse-wave in its transit along



the arterial ways is written out as it passes the different stations, and in association with the cycle of the heart which sends it forth—the heart and pulse at the several arterial points being traced simultaneously. The method by the duplex instrument,<sup>1</sup> in which the movements are traced successively, two by two, throughout the series, gives practically the same results ; but inasmuch as the pre-sphygmic interval and the pulse-wave velocity both vary, evidently, the highest precision in these representations is alone attainable by means of the multiplex instrument.

<sup>1</sup> See *N. Y. Medical Journal*, July, 1877, and February, 1878.

And not only does the method insure this precision, but the multiplicity of the facts it gathers is also remarkable. One experiment by it well made on an average healthy man is sufficient to solve a large part of the problems pertaining to the form, chronometry, and relations of the normal cardiac and arterial movements. Thus, in comparison, while nine separate experiments are required by the duplex method to determine with nearest permissible accuracy the time-relations between the heart and arterial points named, and between the arterial points themselves, one single experiment by the multiplex method shows all these with absolute precision.

Nevertheless, it is indeed fortunate that the simpler combination fulfils all clinical, and much the greater part of physiological, purposes; for this is easily managed, and with practice can be successfully applied by the operator alone, while it is also happy that for exceptional physiological researches the transmission sphygmograph admits of combination in any required number, and the apparatus so formed can be successfully employed with the aid of skilled assistants.

Among the many demonstrations of the experiment in question we here instance the following concerning the pulse successions:

The cardio-carotid time-difference is	.0869 second
The cardio-femoral      "      "      "	.1666      "
The cardio-radial      "      "      "	.1666      "
The cardio-posterior tibial      "      "      "	.2272      "
The carotid-femoral      "      "      "	.0797      "
The carotid-radial      "      "      "	.0797      "
The carotid-posterior tibial      "      "      "	.1403      "
The femoral-posterior tibial      "      "      "	.0606      "
The radial-posterior tibial      "      "      "	.0606      "

The femoral-radial time-difference in this case is *nil*.

Having these figures it only remains to know the arterial

distances between the points named, to be able to compute the velocities of the pulse-wave over the different arterial lines. These distances by external measurements and estimates are the following:

Cardio-carotid	distance,	7 inches.
Carotid-femoral	"	18 "
Carotid-radial	"	23 "
Femoral-posterior tibial	"	33 "
Carotid-posterior tibial	"	51 "
Femoral-radial	"	5 "

Therefore, the velocity of the pulse-wave

between the carotid and femoral is 226 in. per sec.

Between the femoral and posterior tibial, 544 " "

" " carotid and radial 288 " "

" " carotid and posterior tibial, 363 " "

And hence the corollary:

The velocity of the pulse-wave is slowest along the aorta, fastest along the arteries of the lower extremities, and intermediate along the arteries of the upper extremities.

Again the data at hand permit us to approximately determine the duration of the ventricular pre-sphygmic interval. The velocity of the pulse-wave between the heart and carotid must be very nearly the same as that between the carotid and femoral. Accordingly, on the basis of latter and seven inches distance, we arrive at  $\frac{7}{226} = .0309$  second as the time required for the pulse-wave to travel from the aortic orifice to the carotid point. This value deducted from the full cardio-carotid time-difference, namely, .0869 second, as recorded, gives .0560 second as the pre-sphygmic interval, or time comprised between the beginning of ventricular contraction and the opening of the aortic valves.

The experiments on animals and on the schema, notably those of Marey, having for object the elucidation of the

form and successions of the pulse-wave sent by the heart along the arteries, are thus supplemented by the experiment on living man himself, in which the pulsation of the heart and resulting arterial pulses are truly recorded in form and time, and their actual relationships to each other.

## PERIOSTEAL PRESERVATION IN AMPUTA- TIONS OF THE LEG.\*

BY JOSEPH D. BRYANT, M. D.  
NEW YORK.

THE title of this paper is not suggestive of any new ideas, nor will it appear as the reading progresses, that any thing of a revolutionary interest has fallen under the observation of its author. It is not even deserving of the designation, "old wine in new bottles." The only reason, hackneyed though it be, which I have to offer for having selected this theme, is the desire on my own part to elicit a discussion on the practical worth of periosteal flaps, as they are often called, on the part of those who have had a more extended observation of their utility than myself, at the same time to bring forward, incidentally, such practical proof as may have fallen beneath my own notice, bearing upon the manner of making them, their subsequent usefulness, complications, etc., etc. The early history of periosteal preservation, an expression which will be in this instance used synonymously with "periosteal flap," will form but a small portion of this paper. The influence of the periosteum in the reproduction of new bone in the presence of its diseased prototype, is a fact which has created more or less attention since, and even before the days of John Bell, who was familiar with this peculiarity of the membrane. It is not necessary, however, to quote from the

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\*Read before the New York Academy of Medicine, on April 20, 1882.

experience and observation of John Bell, or any surgeon-author, or practitioner of other than our own day and generation, to prove the power possessed by this membrane to reproduce the type of the bone which it normally envelops, protects, and nourishes. The labors of Drs. James R. Wood, Sayre, Markoe, Stephen Smith, Hamilton, and many others in this field, the results of which can be found in the private and public collections of this and other cities, speak loudly in commendation of the judgment and skill which have been shown, and establish positively the reproductive power of the periosteum when associated with diseased bone. The phase of the subject to which I desire especially to call your attention this evening, relates to the ability of healthy periosteum, when separated from healthy bone, to produce bone of a practically normal texture. Much time and not a small amount of labor have been expended upon this branch of the subject, notwithstanding which, as yet, it has not been sufficiently proved by practical observation, to be entitled to be considered an established fact of enough importance to become a part of the woof of surgical teachings and text-books. Many authors allude to it only; others speak of it in a cursory manner, which plainly indicates that they have little confidence and less experience in the matter.

In order to proceed understandingly, let this branch of the subject be divided into several questions. It will serve the important purpose of simplifying and shortening this paper, and at the same time bring the points for discussion directly before you.

First, can healthy periosteum be separated from the healthy bone of the human subject, and its integrity be sufficiently preserved for it to perform its characteristic function—the production of bone? If a belief could be based upon the observations which have been made on the lower

order of animals, this question could, without any hesitation, be answered in the affirmative. The experiments of Ollier on rabbits conclusively proved, that in this animal this membrane, when displaced by transplantation or otherwise, still retained the power of producing bone.

In the human subject these experiments have proved less satisfactory, having been much more uncertain in their results.

That bone has been found associated with the detached periosteum of the human subject is unquestionable, yet, whether it was removed from the bone by the same force which caused the detachment of the periosteum, or whether it developed subsequently from it, has, in many instances, not been satisfactorily determined. I mean when I say satisfactorily determined, determined with that degree of assurance which carries a positive conviction of its certainty. If this question be considered from an anatomical standpoint, and the periosteum be divided into two layers, the external or fibro-vascular, and the internal or cellular layer, often called, according to the fancies of the various authors, "osteo-genetic," "osteal cell layer," "subperiosteal blastema of Ollier," etc., then the answer must depend upon which of these two layers is essential to the reproduction of bone. If the presence of the fibro-vascular layer be only necessary for the reproduction, then success is assured in the beginning, since it can be easily elevated in nearly every instance, ordinary caution only being sufficient to maintain the integrity of its vascular supply. It is different, however, regarding the inner or cellular layer; it, being of a blastemic nature, clings closely to the outer layer and to the bone surface upon which it rests. It does not seem possible that this subperiosteal blastema, which is of a plastic consistency, can be entirely removed from the bone surface, and it is a fact, that microscopical observations have proved it

to be impossible; still, with some portion of the cell layer remaining upon the bone, and the absence of detached bone nuclei on the inner surface of the separated membrane, this membrane has been observed to produce bone. If a goodly portion of the blastemic layer be removed with the membrane, it is probable that bone will be produced, for, as is well known, while the periosteum is the most important, all tissues in contact with bone aid in its development and reproduction, and no one, as yet, has been able to positively assign to each an independent action in what must therefore be considered a common task. The fibro-vascular and cellular layers have, undoubtedly, a co-equal and inter-dependent importance; the chief function of the former being to supply the elements from which the latter elaborates the definite structure. The age of the patient has been found to exert a marked influence on the production of bone from detached periosteum. In early life when the bone growth is rapid, the vascularity of the outer layer is much greater, and the cellular layer is much thicker, than in later life. In adult life, when the dimensions of the bones become established, and repair has but to keep pace with normal disintegration, then the cellular layer is lessened in thickness, the vascularity of the outer layer diminished, and the periosteum becomes more firmly attached to the bone.

Finally, in old age the cellular layer may disappear, then the fibro-vascular layer clings closely to the bone, its function being principally that of protection. It is at this period only, that any great difficulty is experienced in separating the membrane from the bone it surrounds. Ollier in 1864 asserted that one of the conditions especially necessary to success, was a firm, thick, and well-vascularized periosteum. This condition is characteristic of the periosteum of youth, likewise of that which is subjected to the

stimulus of diseased bone; hence the reason why the periosteum of youth and of diseased bone so readily performs the functions of reproduction. The experiments of Langenbeck, Lücke, Stokes, and Ollier have shown conclusively that the membrane which we call the periosteum can be detached from healthy bone, and, after such detachment, bone will be developed from its inner surface of sufficient amount to be entitled to special consideration as an element of usefulness in operative surgery. The success attained is in direct ratio to the age of the patient and the caution observed in the separation of the membrane. In fact, all the tissues in contact with the bone must be treated with the respect commensurate with the importance of the trust to be reposed in them. In view of these facts, this question can be safely answered in the affirmative.

A second and multiple question can now be asked. Is this secondary growth of bone constant in occurrence, durable in existence, and useful to the patient? The experiments of those already mentioned show it to be not constant in its occurrence. The reasons for this are not quite so clear as those showing why it should occur. The rule is, however, that in properly selected cases production of bone does take place; its non-appearance often depending upon the inscrutable reasons which modify or prevent normal actions in other tissues of the body. It is unquestionably true that the active vital processes characteristic of the periosteum of early and middle life exert a most powerful influence in carrying on this function. It is likewise true, that a careful removal of the periosteum, thereby maintaining the integrity of its vascular supply and the attachment of its cellular portion, can not be underestimated. In brief, the injudicious selection of cases, the useless and unnecessary bruising and crushing attendant upon the "poking-up" process, so often witnessed, may de-

stroy its vitality, or so modify its functions, as to bring the operation into disrepute. If proper precautions be observed in the selection of cases and manipulation of the tissues, new bone will be produced within four or five months, which is not, however, *in all instances permanent*. Billroth, in 1868, pointed to the liability of the absorption of the new bone tissue, especially when the subjects were advanced in life or extensive suppuration had occurred. This fact applies equally well to new bone attendant upon necrosis, or developed from the detached periosteum of healthy bone. In young subjects, and where union occurred with little suppuration, he admitted the value of the method. It is proper to notice at this time, that when the new bone was absorbed, the fibrous portion of the periosteal tissue still remained to protect the osseous end in the stump.

In bone forms, is it useful to the patient? This portion of the question can be best answered, by recounting the advantages assumed to be gained by the use of periosteal flaps. They are said to prevent necrosis and atrophy of the end of the divided bone; to prevent the entrance of discharges into the canals; to aid in preventing retraction of the flaps; to prevent the adhesion of the cicatrix to the extremity of the bone; and to provide by the bony growth of a firm extremity, which obviates the danger of a tender and irritable stump. If one half these aims be attainable, the patient must then be inestimably benefited. The influence exerted in preventing necrosis is of an indirect rather than a direct nature. The great care necessary to be used in dividing the bone close to the periosteal reflection, without injuring the reflected membrane, is one of the most important of the preventive influences against necrosis. It is no doubt true, that if undue separation takes place, the displaced membrane will, when replaced in contact with the bone, exert a protective influence and may become reunited to it.

Atrophy and a consequent conicity of the end of the bone is prevented by the maintenance of the nutrition of its divided extremity, through the attachments formed between it and the periosteum reflected over it. The sheltering influence afforded to the end of the divided bone by the periosteal hood, protects it from the discharges arising from the soft parts, thereby being beneficial as a preventive of osteo-myelitis, caries, and purulent absorption. The ability to aid in the prevention of the retractions of the flaps no doubt exists, but in such a slight degree as to be of little practical importance. The power to prevent the adhesion of the cicatrix to the end of the bone is, in my judgment, well founded, and is of itself alone of sufficient importance to merit the closest attention. It may be said that, if sufficient care be exercised in making flaps of proper form and length, and the necessary precautions be taken in their subsequent dressing and care, this very objectionable feature of many stumps will be reduced to a minimum. This assertion, however, does not form a conclusive reason against it as a preventive measure, but rather serves to emphasize the necessity of using every means possible of obviating the occurrence of such an unfavorable result. It is said, that if in the near future, bony spiculæ are to shoot from the flap into the soft parts, to irritate the stump and torment the patient, requiring an operation for their removal, the benefit is gained at too great cost to the patient and the reputation of the surgeon. There are those, however, who, with a full knowledge of the liability to the spicular growths, consider that the advantage gained by securing a movable cicatrix, even at the cost of a second operation, is too great to be relinquished. On examining into the question of adherent versus movable cicatrices, I found more practical knowledge could be gained by consulting with those mechanics who see more of the sequels

of amputations, as revealed by the character of stumps, than the busiest surgeon. I refer to artificial limb-makers, or, as they are more recently called, the producers of "compensative appliances." They soon learn by observation alone, that a stump with an adherent cicatrix is not only a source of annoyance to the patient, but often brings their most persistent and best-directed mechanical efforts to an untimely end, often subjecting themselves to no little vituperation and pecuniary loss.

Now, that something has been said in favor of the plan, your attention will be directed to certain objections that have been urged against it. The unqualified objection has been made that it is impossible to raise healthy periosteum from healthy bone, therefore; the plan must be a delusion and a snare, of no use to the patient, and an annoyance to the surgeon! It is hardly necessary, I think, to again state the reasons on which the first question was answered affirmatively; for, "if healthy periosteum *can* be raised from healthy bone, and its integrity be sufficiently preserved to reproduce bone," this objection can be urged by those only, who have based their conclusions upon improperly selected cases, or upon their belief of the impossibility of raising the periosteum in its reproductive entirety,—that is, of separating the cellular layer from the bone.

A tyro in surgery can easily raise the fibro-vascular layer from the larger bones, together with such portion of the cellular layer as may cling to it; provided, the subject be not too aged. If it be impossible to raise the periosteum in its reproductive entirety, this assertion against the futility of the attempt holds good only in so far as it bears upon the advantages claimed for the new bony growth; for the membrane, as will be shown, will still protect the bony canals from the discharges, nourish the bone end, and prevent an adherent cicatrix. The second objection, and

seemingly a pertinent one has been made, that at some time, more or less remote, small bony spiculæ will protude into the extremity from the detached periosteum, cause much annoyance, and finally require operative interference for relief.

One of the strongest exponents of the method, in reply to a question of mine bearing upon the liability to this contingency, said: "In but one or two of those cases which I have been able to follow for four or five years, have the bony spiculæ occurred, and in each instance have required surgical interference for their removal; yet" he added, "I consider the advantages gained in these cases to be a sufficient recompense to the patients for the additional trouble." Assuming that all the advantages claimed for the flap are to be realized, still, this objection has much force with those who have a natural timidity of assuring their patients of the exact state of things, which of course must be done in self-defence, if for no other reason; for, all secondary interference will, unless the liability be explained at the time of the original operation, create distrust in the minds of patients, and a disdain for the surgeon on the part of those who have secured serviceable stumps minus the periosteal flap. If its assumed advantages and subsequent complication be explained to the patient, he is quite likely to elect to take his chances without the periosteal flap, rather than to lay a foundation for a heritage of pins and needles, requiring after treatment for relief. About two years ago I had occasion to re-amputate the leg of an army officer, 40 years of age, at the lower third, on account of an adherent cicatrix which caused so much irritation as to render the limb useless for the attachment of an artificial appliance. The question of a periosteal flap was placed before him.

Its advantages and objections were considered, and he decided to let me do as I saw fit. It occurred to me that if

the bony spiculæ protruded into the end of the stump, that it must be due, assuming the cellular layer to be the bone-producing layer, to the periosteum becoming turned outward, or in some manner presenting its bone-producing surface toward the flaps covering the extremity, instead of remaining apposed to the divided bone surface. I had, moreover, on various occasions witnessed the elevation of the periosteum, or, rather its being pushed upward on all surfaces of the bone into a wad rather than a flap, and concluded that the crushing and mixing of its surfaces, due to the illogical force, to be the active cause of the thorny sequel. Often, however, greater care was taken, and the membrane was removed in conjunction with its super-imposed tissues. In the case in question a periosteal flap was determined upon, but, instead of removing the membrane from all the surfaces of the tibia, it was taken from its broad subcutaneous surface. A moment's examination will show that if it be removed from this surface it will possess ample width to cover the divided extremity of the bone. (See fig. 1.)

No difficulty was experienced in removing it along with the super-imposed soft parts forming the flap. It was only necessary to dissect up the soft parts for about an inch, then divide the periosteum transversely, and along the borders of the tibia bounding its subcutaneous surface, when with the handle of a scalpel the membrane was readily pushed upward in conjunction with the soft parts resting upon it. The remaining portions of the flap were dissected up as if no periosteal preservation had been contemplated. In this instance the so-called hood-flap was made, and of course united antero-posteriorly. This did not entirely please me, for it will be readily seen (see fig. 1) that with such a flap united antero-posteriorly, the union will prevent the equal application of the periosteal lining to the divided

end of the bone. Its inner portion will be quite well apposed to the bone, while its outer portion will assume a more or less vertical position; therefore, the flap, as a whole, will not be in a proper position, consequently interfering with union, increasing the amount of suppuration, thereby lessening its bone-producing power, and predisposing to eversion and possible bony spiculae. Any form of flap united antero-posteriorly will, owing to the difference in the direction between the line of union and the direction of the subcutaneous surface of the bone from which the periosteum is raised, cause more or less tilting of that tissue. (See fig. 1.) If the membrane be separated independently of the flap this will be obviated, but at the possible expense of the integrity of its circulation and a greater danger of displacement and eversion. It is true this periosteal flap might be stitched over the end of the bone, but, to do this is to interfere with the freedom of the discharges occurring between the bone and periosteal membrane. This case made a rapid and satisfactory recovery, and the patient has had up to the present time a most serviceable stump.

The second case was an amputation at the lower third of the leg of a patient 50 years of age, for malignant disease of the foot. In this case the circular flap was made, instead of the hood, as in the former. The periosteum was raised, in conjunction with the soft parts, from the subcutaneous surface of the tibia, precisely as in the preceding case; but the co-apтation of the flaps was made obliquely, that is, in a line parallel with the subcutaneous surface of the tibia (see fig. 1), and, therefore, in a line parallel with the line of attachment of the periosteal flap to the bone from which it had been raised. This deviation allowed the upper portion of the flap, with its periosteal lining, to fall by its own weight directly and evenly over the divided end of the bone. (See fig. 2.) This oblique method of co-apтation, not only served

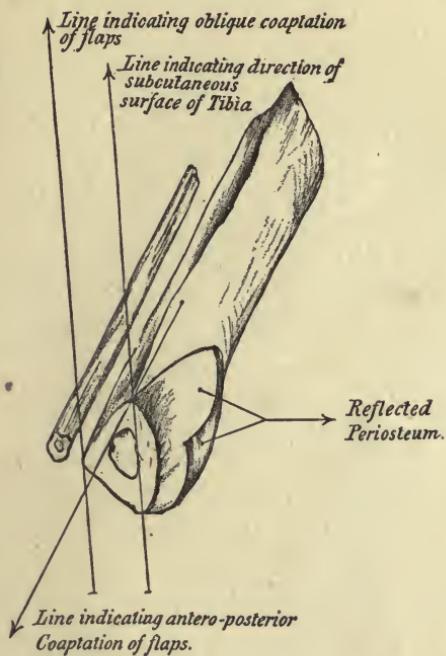


FIG. 1.



FIG. 2.

to properly adjust the periosteum over the end of the bone, but brought the line of union between the ends of the two bones; thereby doubly increasing the importance of the oblique co-apтation. The general principles applicable to the after-treatment of amputations were employed in each case. For obvious reasons, great care was taken to not allow the drainage tube to pass between the periosteum and the end of the bone to which it was apposed. The recovery from the operation was rapid, complete, and not attended by much suppuration. The patient was able to wear an artificial limb five weeks after the amputation, without any discomfort referable to the extremity of the stump. The cicatrix was perfectly movable, and pressure could be borne directly upon the end of the stump without causing any annoyance. Unfortunately, however, this stump too soon became typical of those "blessings that brighten as they take their flight," for, four months after the first operation, it became necessary to re-amputate at the knee joint for a return of the malignant disease at the middle third of the leg. It is unnecessary for me to say to you that the specimen was cherished with due care, and is now presented for your examination. It has been carefully examined by Prof. W. H. Welch and myself, with a view of ascertaining the following facts:

- 1st. The relation of the cicatrix to the end of the bones.
- 2d. The relation of the periosteum to the end of the tibia, and to the soft parts.
- 3d. If bone had been produced, and if so, its relation to the surrounding parts.
- 4th. The condition of the end of the tibia.
- 5th. The condition of the end of the fibula, which had not been covered by periosteum.

On longitudinal section, the line of the cicatrix was

scarcely discernible in the soft parts. (See fig. 3.) It was freely movable, and the integument was separated by a cushion of fat from the periosteum covering the end of the tibia. The periosteum was found to be firmly and evenly attached to the end of the tibia, sealing its extremity thoroughly. It could be detached from the end of the bone with but little difficulty, its continuity with the periosteum above was plainly to be seen, and its relation to the superimposed soft parts was unchanged, being similar in all re-

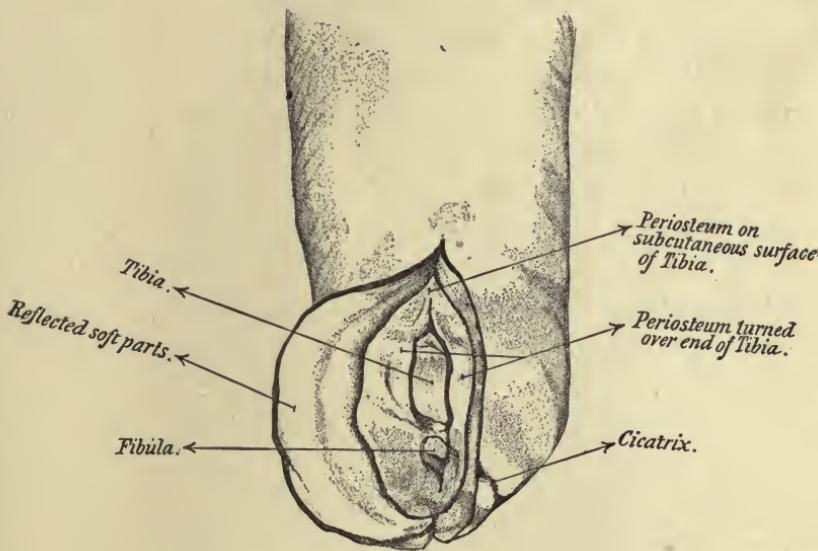


FIG. 3.

spects to that found above. There was not tangible evidence of the production of bone, although the attached surface of the membrane to the end of the tibia was rough and had minute particles of bone connected with it. These were considered to have been detached from the bone during the removal of the periosteum. The end of the tibia was but slightly rounded, due to very limited absorption of its borders, and was closely covered by the membrane. The small dark points showing the extremities of the Haversian canals

were numerous and distinctly marked, the whole thickness of the bone appearing to be composed of moderately compact bony tissue. The relations between it and the membrane were intimate, simulating in all respects those existing between bone and its periosteum elsewhere. The end of the fibula was covered with a fibrous tissue apparently continuous with its structure and that of the surrounding tissues. This specimen, in my opinion, teaches the advisability of periosteal preservation, and practically substantiates the reasons which have been advocated in its favor.

If stumps can be made which present the features of this one, it is a matter of little importance whether bone be produced or not. I am well aware that one opportunity to examine a specimen of this nature cannot be sufficient to establish a rule of faith or practice. These opportunities are, however, exceedingly rare, and I think their results should be made known at once, in order that the good that may arise shall increase their number, or that the evil coming therefrom shall check the desire that gives them birth.

## A CASE OF TUBERCULAR LEPROSY ORIGINATING IN CONTAGION.\*

BY I. EDMONDSON ATKINSON, M.D.,

PROFESSOR OF PATHOLOGY AND CLINICAL PROFESSOR OF DERMATOLOGY IN THE UNIVERSITY OF MARYLAND, SCHOOL OF MEDICINE.

THE history of the following case of tubercular leprosy is reported to this Association, rather on account of its important etiological bearings, than in consequence of any clinical features of special interest.

Mrs. B., 40 years of age, applied for relief at the out-patient department of the University Hospital, October 18, 1880. She was of medium size, had blue eyes and light hair, and gave the following history: Her parents came from Hanover, where they were born, many years ago. The father died of "consumption," the mother of "change of life." She has two brothers and two sisters, in good health. Several brothers and sisters died in infancy, of diseases the nature of which she does not recollect. One brother died at 47 years, of dropsy. The patient was born and raised in Baltimore. In her sixteenth year she married and went to the Eastern Shore of Maryland to live. Her husband kept store in Salisbury, Worcester County. They returned to Baltimore after several years, and remained until the beginning of the late war, when they again moved to Salisbury. Nine years ago they again came to Baltimore, where they have resided ever since. It will thus be observed that Mrs. B. has never been beyond the limits of the State of Maryland.

She has had nine children, but no miscarriages. Five children still live. Those who died were carried off by disorders incident

\* Read at the 5th annual meeting of the American Dermatological Association at Newport, August 31, 1882.

to infancy or childhood. While living at Salisbury, she ate much salt meat, and but little fish. She has suffered from malaria, but not during the past nine years, though living in a malarious locality. Her condition of life has been fairly well-to-do, and her general health pretty good until five years ago, when she began to feel unwell. She was nervous, often had sick headache, pain in the back, and was often feverish. It was not, however, until May, 1878, that she noticed any cutaneous disorder. At this time she observed, while pregnant, yellow spots upon her thighs. Three weeks after the birth of her infant, similar spots were noticed upon the trunk, and in a short time were pretty generally distributed, and became of a much brighter reddish color.

At this time her hands and feet were swollen and tender. She could stand with difficulty. Yellow spots have been copiously present upon her trunk, head, and extremities ever since, though they are not so intensely colored as formerly. Her ill-health continued, but the spots remained the only eruption until the present year (1880), gradually changing to a deep salmon color. Last spring Mrs. B. noticed some lumps upon her face, and shortly afterward upon her arms. Previous to this, during the winter, a bleb as large as a hickory-nut appeared upon the fibular side of the right foot. It gave much pain, and healed slowly. This was the only bleb the patient had had.

Mrs. B. became pregnant toward the end of April, and since that time the nodules rapidly increased in numbers and extended to nearly every part of the body, remaining, however, scanty upon the trunk. Since the beginning of her pregnancy, she had felt wretchedly uncomfortable, and consulted quite a number of physicians, who, however, failed to afford more than temporary relief.

At the date of her visit, her condition was carefully noted. Over the neck, throat, breast, back, arms, feet, and hands (faintly upon the thighs and legs) were flat, circumscribed patches of irregular outline, without elevation and free from desquamation. They varied in size from that of a split pea to that of the palm, frequently becoming confluent. These spots were almost entirely pigmentary. Their color was of a dull, dirty, pinkish-brown hue or salmon color. Over the front part of the neck and face, this coloration was diffusely spread, and involved the entire surface. While these pigmented areas had no elevation, the skin involved in them was very evidently thickened; in some places, especially about the neck and shoulders, it was twice as thick as normal.

This thickening was sharply limited by the areas of pale brownish pigmentation. The portions of skin between these patches were of normal thickness. Upon the right cheek was a dull, dead-white spot as large as the nail of the little finger. This had made its appearance within the last two years, and closely resembled a scar. The finer natural lines of the skin of the face and neck were less distinct than normal, but the coarser lines and wrinkles were exaggerated.

The palms and soles were of a dull yellowish color, and had a glazed appearance. Indeed, the general surface had a muddy look, and, even where no spots were visible, the eye received a dim impression of mottling.

Mrs. B. asserted that these spots were not permanent, but that, after remaining a while, they slowly faded and disappeared, presently to be succeeded by new ones irregularly distributed. The feet and legs were very oedematous. The dorsal surfaces of the feet were puffed and cushion-like, and had a shiny appearance. The backs of the hands were similarly affected. The extremities had been in this condition for nearly two years, and locomotion had been very painful.

During the preceding spring, small nodules began to be discovered over her face as already noted, and rapidly increased in size and numbers. When first seen by me, they varied from pin-head size to pea size, and were scattered in very great numbers over the face, neck, arms, forearms, hands, thighs, and legs. Upon the trunk were but few, situated in the infra-clavicular region. Large numbers of these nodules projected from the skin, but multitudes (of smaller size) could be felt imbedded, though invisible, particularly in the subcutaneous tissue of the thighs. Those that were visible were paler than the surrounding integument or were darker, or, occasionally, of livid appearance. Their surface was smooth and shiny, and their structure was rather dense. They were often seated in the macules. Nodules could be felt deep under the skin of the soles.

There was considerable thickening of the eyebrows and lobes of the ears, but this seemed to depend rather upon diffused thickening than upon distinct nodules, although numbers of the latter were present in these localities. The characteristic facies of leprosy was distinctly foreshadowed.

Sensation had been decidedly modified since the beginning of the complaint. There had been no itching, but, rather, a pronounced numbness. This had never been fixed and permanent,

but parts that had once been benumbed, had recovered perfect sensibility after a while, new areas of numbness appearing from time to time. There had never been complete anaesthesia. The point of a pin could be felt wherever pressed into the skin, rather more painfully upon the unaltered portions than upon the macules. As tested by the aesthesiometer, however, diminished sensibility seemed rather to be encountered in certain areas, irrespective of the distribution of the macules.

A rhinoscopic and laryngoscopic examination kindly made for me by my friend, Dr. H. Clinton McSherry, revealed thickening and granulation of the mucous membrane of the posterior nares, and on the left side of the anterior nares. The epiglottis was seen to be slightly thickened, but the two arytenoid cartilages and the inter-arytenoid fold were very much thickened. Weakness of the eyes was complained of, but this was found to be due to conjunctival hyperæmia, her vision having been ascertained to be normal by my colleague, Prof. Chisolm.

Her functions were, for the most part, naturally performed. Heart and lungs were healthy. Urine was normal in quantity, and free from albumen and tube casts, but with a copious deposit of lime-oxalate octohedra. Her temperature was 99° F. (She was at the time of this examination about six months pregnant.)

As Mrs. B. approached the termination of her pregnancy, multitudes of small tubercles appeared over her face, neck, buccal and faacial mucous membrane, and extremities. She would have attacks of fever, which would keep her in bed for several days, and upon their subsidence a notable increase in the number of tubercles would be observed. The œdema of the legs markedly increased. Many tubercles became livid in color and, softening, broke down into small excavated ulcerations. In numbers of the tubercles, disintegration took place in a peculiar manner. Each would become surmounted with rather large vesico-pustules, the ulcer appearing after the rupture of these. The number of these tuberculo-pustules increased rapidly during the last weeks of her pregnancy, and the mucous membrane became invaded by them. The mouth (tongue, hard and soft palate, gums and inner surfaces of the cheeks) became the seat of great numbers of them, speedily passing into superficial ulceration.

At the date of her confinement there were distributed over her person several hundreds of these small ulcers that had resulted from tubercles. The eyebrows, lips, and ears, had now become de-

cidedly tuberous. The child was a healthy boy. After her confinement Mrs. B. improved rapidly. By February 28, many ulcerations had entirely healed, leaving scars. Very many small nodules had disappeared without suppuration, and many could be observed in process of involution, becoming smaller, losing their hyperæmia, and becoming deeply pigmented. Most of them disappeared in this latter manner, and the hand, passed along parts of the skin where they had been most abundant (inner surface of thigh, for example), could now perceive almost nothing abnormal. New nodules continued to appear, however; many of the old ones remained unchanged; the thickening of the skin and the maculations remained pretty much as before, and the face was gradually assuming a more characteristic expression.

The case up to the present time has been slowly progressive, shows now, August 15th, all of the above-mentioned symptoms in a somewhat exaggerated manner; the ulcerations, it is true, are nothing like as numerous as they were just previous to her confinement, but they have become numerous and intractable on the lower extremities, and occasion much distress. The fingers and toes show, as yet, no characteristic changes, but look somewhat clubbed. Both ulnar nerves can be felt, enlarged and thickened. The general condition remains about as usual. The last baby is being nursed at the breast and remains apparently healthy, as does also the one now about 2 years old, who was born subsequently to the appearance of the mother's leprosy.

I have given the preceding history at length, at the risk of proving tedious, not because any unusual or striking features became manifest, but in order to make it clear that I had to do with a perfectly well-marked case of tubercular leprosy, in view of the very important etiological aspects of the case. After careful interrogation I had failed to discover any causative influence whatever, and was beginning to conclude that it was a purely sporadic leprosy of undiscoverable origin, when one day Mrs. B. remarked that her neighbors were beginning to notice that her appearance was assuming a resemblance to that of a man named Brown who had lived in the immediate vicinity some years previously.

I soon ascertained the fact that this Brown was the same individual whose case was reported as one of tubercular leprosy, in the *Maryland Medical Journal* for July, 1878, by Dr. George H. Rohé,<sup>1</sup> and easily obtained an account of the movements of my patient since her return to Baltimore in 1870.

After living for one year in a neighboring street, she moved to No. 139 Ridgeley Street, where she lived two years. At the time of this removal, the man Brown lived at No. 113 of the same street, or thirteen doors further up. The families became intimate, and Mrs. B. was once or twice in the house of Brown, visiting his wife. During the next year, however, Brown removed to No. 141 Ridgeley Street, or next door to the house occupied by Mrs. B.'s family. Here Mrs. B. occasionally saw Brown, but never had any thing to do with him, never even shook hands with him, nor ate any thing in his house. He had been in her house, but had never eaten a meal there. After Brown had been living at No. 141 for one year, the B. family removed to their present residence, No. 102 in the same street, just opposite their old home. Brown moved out of the neighborhood four or five years ago. (These houses are small two-storied structures, of about 15 feet frontage, and, with a small back building, containing five or six small rooms. They are in immediate juxtaposition.)

At the time she first became acquainted with Brown, he had eruptions and nodules upon his face and was in bad health, and was said to have suffered from frequent attacks of "erysipelas."

Mrs. B.'s children appear to be perfectly healthy, and those born since she became leprous remain, as yet, without a trace of their mother's disease. Her husband is a healthy laboring man, with no evidence of disease.

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<sup>1</sup>The portrait of this man appears in Dr. George H. Fox's "Atlas of Portraits of Skin Diseases" as a representation of tubercular leprosy.

During the period of its greatest prevalence in Europe, in the eleventh, twelfth, thirteenth, and fourteenth centuries, leprosy was universally believed to be contagious; and at the present day, in those places where it is most frequently encountered, the popular belief in its contagiousness remains unshaken. Although the disappearance of the malady from the greater part of Europe has been attributed to the isolation enforced upon lepers during generations, rather than upon any especial change in climate, habits, diet, or in the general condition of the people, the views of authorities seem to have, during late years, tended toward a disbelief in the contagiousness of leprosy, and most of those who have devoted the greatest study to the subject attribute its origin to other causes. Thus, Danielssen and Boeck, Virchow, Hebra, and others have thrown discredit upon the theory of contagiousness. The profession generally, however, does not appear to have been convinced, and those who deny absolutely that leprosy may spread by contagion are few. A few writers, indeed, strenuously maintain that the disease is disseminated in no other way.

Well-observed cases have been reported in abundance, where the disease could be directly traced to contagion; cases where the histories have been followed up with so much skill and intelligence that one could hardly fail to be convinced of their genuineness, were it not that they have always occurred in countries where leprosy has been known to prevail, where other causative influences might have been brought to bear, and where all persons were exposed to similar influences depending upon peculiarities of climate, soil, diet, habits of life, etc., to which the spread of leprosy has been variously attributed. Science demands, before the contagiousness of leprosy can be admitted as proven, that the evidence must be produced in countries where the endemic influence does not prevail, where lepers introduced

from other parts shall have communicated the disease to persons with whom they may have been brought into contact. The difficulty of fulfilling such requirements are undoubtedly great, but it would seem that in this country opportunities may fairly be expected to occasionally occur, since leprosy, where it prevails, has, with one or two exceptions, undoubtedly been introduced from without; and since in many sections the disease has never been known. Says W. Boeck (Neumann: "Lehrbuch der Hautkrankheiten," 5 ed., 1880, p. 54): "The United States of North America must be looked to as the fittest place for the elucidation of this question, since leprosy does not prevail there, and since there are known no other influences that have been considered the causes of leprosy."

The case that I have given in detail in the preceding pages fulfils, I think, all the requirements of the question. So far as I have been able to ascertain, no cases have ever been reported, where leprosy has originated within the borders of the State of Maryland. It is true that Munro, whose series of papers in the *Edinburgh Medical Journal* on "the etiology and history of leprosy" are decidedly the ablest presentation of the question, from the standpoint of a contagionist, writes: "It has been stated to me that cases" [of leprosy] "occur in the Southern States of America, and such cases are seen among the blacks at Baltimore, but whether of blacks from the West Indies, or natives of the States, I have no information" (August, 1877, p. 145). He does not, however, give the sources of his information, and it is probable that it was derived from rather vague statements of not very competent observers, or the references would have been given. Certain it is, that in the sense implied, leprosy is not met in Baltimore, either among whites or blacks. That cases of leprosy may have, at one time or another, been present in Baltimore without having been re-

corded is quite possible, but the fact remains that the vast majority of medical men in the city and State have never even seen a case of leprosy; nor with the exception of the cases I am about to refer to, has leprosy been treated in the city hospitals for a great number of years. The only cases of the malady that I can find any reference to, were reported to the *Maryland Medical Journal*, vol. iii., p. 147, by Dr. George H. Rohé. These were three cases of tubercular leprosy.

One patient was a young man, a native of the West Indies (Barbadoes), of English parentage. He became leprous at the age of five years. His parents were both healthy. He came to Baltimore when 15 years old, and was admitted into the University Hospital about 1870, where he was under the observation of Prof. Tiffany, who communicated the notes of the case to Dr. Rohé in a letter. This man subsequently died at the age of about 22 years. A second case was that of a woman, born in Baltimore of parents who never became leprous. At the date of Dr. Rohé's observation she was 46 years old, and the mother of four children the oldest 29 years old, the youngest 8 years old, all in good health. In the spring of 1855 she went to New Orleans, where she remained until the occupation of the city by the Federal troops during the late war. Subsequently, desiring to be near her husband, who was in the Confederate service, she resided in different parts of the South. In 1865 she returned to Baltimore, where she had since resided. Symptoms of tubercular leprosy appeared four years after her return from the South, and subsequently to the birth of her youngest child. This patient died in 1878, shortly after Dr. Rohé's report. When last heard from by Dr. Rohé, the child remained healthy.

The third patient reported by Dr. Rohé (the first mentioned in his paper) was the one with whom my patient

came into contact, and I need no apology for quoting the case somewhat fully. This man, A. B., was born in New York City, and was 54 years old. He was by trade a brick-layer. His parents had been free from leprosy, and he had two sisters who remained healthy. "In 1855, he went to Cuba, working for a gas company in St. Iago for 9 years." His wife, to whom he was married 21 years ago, joined him in Cuba, with their two boys, five years later. "In 1864 he left St. Iago for Baltimore, and has since then been living here, working at his trade until last summer" (1877). He dated his first symptoms back two years, but from the account given by my patient, it is most probable that he had manifested symptoms of the disease earlier. For already in 1872, they were next-door neighbors, and at that time B. is said to have had spots on his face, and to have suffered from frequent attacks of "erysipelas."

It is not now claimed that these are the only cases of leprosy that have ever been known in Baltimore. Indeed, as I have already remarked, it is altogether likely that occasional cases have been seen there, but I have been unable to discover any notice of such. At all events, I think I have made it clear that leprosy is one of the very rarest of diseases in Baltimore and Maryland, and is practically unknown to the medical profession there.

I conclude from all the foregoing considerations, that it is in the highest degree improbable that the only case of leprosy that has been reported as having certainly originated within the State should have developed, without recognizable predisposing influences, a few years after having been next-door neighbor to one of the only three lepers that have been observed, so far as I have been able to ascertain, in a city of 400,000 inhabitants for a period of, at least, many years, without having in some manner derived it from him. It is hardly possible to imagine such a coincidence as the result of accident.

Especially important, in relation to the question of the contagiousness of leprosy, are the recent discoveries of Armauer Hansen (*Virchow's Archiv*, 79 Bd., 1880), Eklund (Stockholm, 1879), and Neisser (*Breslauer Arztlichen Zeitschr.*, Nos. 20 and 21). The detection, by these observers, under the guidance of Hansen, of the bacillus lepræ cannot fail to exert a decided influence upon the future history of leprosy. With a view to seek a confirmation of the results of these writers, the entire lobe of the right ear of my patient was removed with scissors, and submitted, for histological examination, to my friend, Dr. I. Bermann, who has reported his conclusions in the accompanying paper.

I cannot close this paper without referring to the national importance of settling this question of the etiology of leprosy. In no country in the world is definite information upon this subject so urgently demanded as in ours. With Chinese lepers pouring in upon our Pacific coast, with Norwegian lepers settling in the Northwest, with an increasing number of lepers in the State of Louisiana, we seem to be threatened at many points. Should the disease prove to be contagious, as from an unprejudiced consideration of my own patient I am convinced it is, there can be no subject more worthy of the earnest consideration of our sanitarians and legislators.

## THE BACILLUS LEPRÆ.

BY I. BERMANN, M.D.,

BALTIMORE, MD.

**I**N May, 1881, my friend Dr. I. E. Atkinson asked me if I would like to confirm, by microscopical examination, his clinical diagnosis of leprosy on one of his patients who had consented to have a piece of skin excised for that purpose. Our object was to see whether the discovery made by Hansen, Klebs, Eklund, Neisser, etc., as to the existence of the bacillus lepræ, would be verified by us, and thereby prove beyond doubt the existence of a solitary case of leprosy in this city. The literature of this subject is, considering the rarity of the disease, pretty extensive, especially as regards the pathological anatomy, and the histological changes occurring during its progress. To those who wish to refer to this literature I may say that the last paper of Dr. H. D. Schmidt, of New Orleans, published in the *Chicago Medical Journal and Examiner*, April, 1882, gives all necessary points on this subject.

It is not the object of this paper to enter into details of the pathological anatomy of lepra, especially as Dr. Schmidt has lately given such an exhaustive and exact description of it (*vide ARCHIVES OF MEDICINE*, Dec., 1881), with which, on the whole, I can fully concur..

The first author who called attention to the fact that rod-like organisms can be found in leprous tissue, blood, pus, etc., and that they bear a direct relation to the disease,

was Hansen. After him, Klebs and others have published similar results from their investigations of leprosy. These papers have not attracted the attention they deserve, as we find no mention of them even in the newest editions of our hand-books on skin diseases.

The discoveries of Weigert, Koch, Ehrlich, and others, regarding the use of aniline colors in examinations for bacteria enabled Neisser to make use of their methods in his investigations on the pathogenesis of leprosy, and the results of his researches have established, beyond doubt, the existence of bacillus lepræ. Not only was he able to detect and demonstrate the bacillus in all his cases, but also to cultivate them in blood serum and other fluids (*vide Virchow's Archiv*, June, 1881).

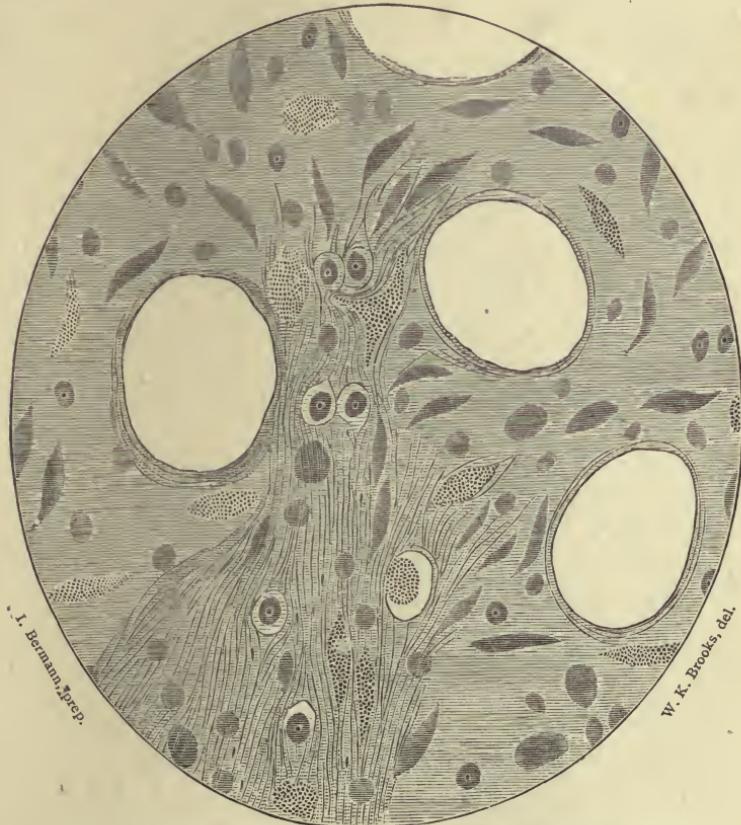
Dr. Atkinson and I took the piece of leprous tissue, submitted to my examination, from the lobe of the ear of the now living patient. It was hardened partly in Müller's fluid, partly in alcohol.

The sections were made after my dry-cutting method, and transferred first to turpentine, and then to absolute alcohol, in which they were kept constantly. After employing the usual methods of staining, without satisfactory results, I finally got a glimpse of the bacillus in the specimens by following the procedure described by Neisser in the aforementioned paper, and that of Weigert in the May number of the same journal. Neisser recommends that the sections be treated first with liq. potassæ (1:12), which, according to him, shows the bacilli without staining; but better results are achieved by using gentiana, methyl violet, or fuchsine, a one-per-cent. solution of these coloring agents being employed. The sections are subsequently washed in acidulated alcohol. Neisser expressly stated, however, that bacillus lepræ stains with more difficulty than any other bacteria; he likewise recommends an acid eosin-

hämatoxylin solution as especially effective for the demonstration of the bacillus lepræ.

I have been able to verify Neisser's results by these methods, but have derived more satisfaction by following the plan recommended by Weigert, which is to transfer the section from distilled water into a one-per-cent. solution of aniline blue or purple (the aniline blue which I used comes from Vogler, Son, & Co., of Baltimore) for a few seconds, and then washing it in distilled water. It is then transferred to absolute alcohol, which takes a large part of the color out again. This alcohol must be changed several times. The section is next placed in oil of cloves. By putting the section alternately into alcohol and oil of cloves, the coloring matter is almost entirely removed from the cell protoplasm, so that only the bacilli contained in the section are stained bright blue or purple. It is finally mounted in Canada balsam or damar varnish, after having remained in oil of cloves for about twenty-four hours. Sections prepared in this manner, and examined with a good  $\frac{1}{8}$  or  $\frac{1}{10}$  objective system and condenser, will show the bacillus most perfectly. The superior advantages of Weigert's method became apparent immediately upon examination. The groups of large cells, which constitute the special feature of leprous tubercles, appear to have retained more of the staining fluid upon superficial examination. On submitting them to a higher magnifying power, it becomes apparent that the intensity of color is due to the presence of deeply pigmented rods within the cells, and not simply to the staining of nuclei. The size of these rods equals  $\frac{1}{6}$  to  $\frac{1}{8}$  the size of a red blood corpuscle in breadth, and  $\frac{1}{2}$  to  $\frac{1}{3}$  in length. Almost without exception they are found most perfectly within the large "lepra cells" (Virchow), and irregularly distributed through them. These cells are usually four to eight times the size of a white blood cor-

puscle, and are lying close together with but little interstitial tissue. If the specimen is stained double, with eosin and violet, there will also be found distributed among the larger cells mentioned a few cells resembling the Waldeyer's plasma cells, which stain intensely red with eosin, do not take the violet staining, and never contain bacilli. The larger cells contain frequently several nuclei. Very often I find very deeply stained large cells, which on close examination prove to be filled almost entirely with bacilli of different sizes.<sup>1</sup>



Section from lepromatous tissue showing rod-shaped bacteria—*Bacillus lepræ*.

<sup>1</sup> As a curious coincidence I should mention that in some of the lymphatic vessels I find networks of fibrinoid substance containing at the same time bacilli. The same observation can be made in the lymphatics in syphilitic tissue, and in tuberculous tissue.

These large cell conglomerations I always find immediately beneath the rete Malpighii, and, cannot discover among them any blood-vessels. On that account I should consider them as tubercles, with highly developed cells about to undergo a rapid disintegration for want of proper nourishment.

The *Annales de Dermatologie et de Syphiligraphie*, October 25, 1881, contain a paper by Cornil and Suchard on the same subject, with very excellent plates, so that it seems almost unnecessary to add a plate to this paper, except that the one drawn for me by Prof. W. K. Brooks, of the Johns Hopkins University, is a correct copy of one of my sections.

In conclusion I wish to call attention to the fact that only a very strict observation of the rules given will enable investigators to achieve satisfactory results, and that, even in spite of most careful manipulation, specimens are liable to fade within 24 hours of their preparation.

Since the above was written I have found that by staining the specimens first either in a 1% eosin or Bismarck solution, before staining with the aniline blue, still better results can be obtained.

In the April number of the *Chicago Medical Journal and Examiner*, Dr. H. D. Schmidt, of N. O., discusses the question whether the bacillus lepræ is a reality or a fiction. That he has not succeeded in finding them in his specimens I cannot doubt, since it was testified that they could not be seen in them by microscopists in Chicago. I am satisfied that some fault in his method is alone the cause of his non-success, and I should be very glad to stain some of his material if he will send it to me, and believe that I could convince him, in this way, of the unfictional character of the bacillus lepræ.

## A SUCCESSFUL CASE OF LIGATION OF THE COMMON CAROTID ARTERY WITH A CARBOLIZED NERVE.

BY JOHN A. WYETH, M.D.,

VISITING SURGEON TO MT. SINAI HOSPITAL, CONSULTING SURGEON TO ST. ELIZABETH HOSPITAL,  
NEW YORK.

THE re-introduction of the broad animal ligature into surgical practice, and its present popularity, are due to the brilliant surgery of Mr. Richard Barwell. Before ligatures were made antiseptic by being carbolized, various kinds of animal tissue had been experimented with, adopted, and later abandoned.

In 1814, Physick<sup>1</sup> used chamois skin. Hartshorn preferred parchment. Jamieson, of Baltimore, tied the carotid, iliac, femoral, and other smaller arteries, with flat buckskin strings. No secondary hemorrhage followed, and the ligatures (which were cut short and left in the wound) were never heard of. The wounds healed generally by first intention.

Astley Cooper used the ligature recommended by Physick, but condemned it later.

Travers, Lawrence, Cawardine, Porta, and others, in addition to the materials just mentioned, used ligatures made from intestine, nerves, tendons, and raw hide.<sup>2</sup> All of these

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<sup>1</sup> Cooper's "Surgical Dictionary," p. 195.

<sup>2</sup> Prize Essay of the American Medical Association, on "Treatment of Aneurisms," B. Howard, M.D. Printed by Collins, Phila., 1870, p. 25.

fell early into disrepute—gave way to silk and metallic ligatures, and did not appear in surgical literature until within the last few years. Carbolized sheep-gut threads twisted hard, small, and round, were brought prominently forward as safe and serviceable animal ligatures more than a decade ago by Mr. Lister, and have grown in professional favor.

In 1880, Mr. Richard Barwell published his cases (up to that date) in his booklet, "On Aneurisms; especially of the Thorax and Root of the neck."<sup>1</sup> The success he achieved was so remarkable and gratifying, that I determined to use his ox-aorta tape ligatures in a case which had come under my care, and on September 21, 1880, I tied the right common carotid, and the right subclavan, for the relief of a large aneurism of the ascending segment of the aorta.<sup>2</sup> The relief was immediate and marked. The patient improved for several months, the aneurism decreasing in volume.

In April, 1882, she exposed herself to unusual fatigue in very inclement weather, and contracted a violent bronchitis, which confined her to bed for weeks. The aneurism enlarged again, but never resumed its former size, nor did the patient suffer from it as she had done before the operation. In August, 1881, she was attacked with a severe diarrhoea, from which she died in the latter part of that month. With the kind assistance of Drs. Converse, King, and Cramer, I made the autopsy. Both arteries were completely occluded, and there was no remnant of the ligature, which had left a more noticeable indentation upon the subclavian than the carotid.

The subclavian was occluded only for a distance of about one half an inch. The collateral circulation was unimpeded in the internal mammary, thyroid axis, and other branches of this artery. There were some objections to the ox-aorta

<sup>1</sup> Macmillan & Co., London, 1880.

<sup>2</sup> *American Journal of Medical Sciences*, January, 1881.

ligatures, as I had prepared them. The knots were too bulky, and it was difficult to obtain them.

In looking about for a smoother and stronger substance I determined to use nerve tissue. I concluded that it was probable this material had been used before, although I had not heard or read of such a procedure until seven months after my operation, when, in looking up the subject for this article, I found (as heretofore quoted) that threads had been used as ligatures, which were made of "tendons, nerves," etc. I regret that I cannot give the name of the first experimenter, or his case or cases.

My own operation was the following.<sup>1</sup>

Ellen W., æt. 53, Ireland, married, domestic, several children. Admitted to Mt. Sinai Hospital May 2, 1881, for malignant disease involving the right antrum maxillare, spheno-maxillary fossa, cavity of orbit, and the frontal, sphenoidal, and ethmoidal sinuses. The right eye had been completely destroyed. For several months she had had severe headaches. In June a sharp hemorrhage occurred into the mouth and nares while she was sleeping. She awoke in time to prevent suffocation. The pains and occasional hemorrhage continued until the 27th of September, when I tied the right common carotid below the omohyoid, with the freshly carbolized sciatic nerve of a calf. My object was to arrest the hemorrhages, retard the growth of the neoplasm, and relieve the severe pains due to its presence. The ligature was cut off and left in the wound, which healed by first intention. The pulsation never returned in the temporal of the right side. The pains on the affected side ceased, but the increase of blood pressure on the opposite side caused "a singing in her head," and a disturbed feeling which was painful. These symptoms gradually subsided. The deeper portions of the neoplasm began to break down and suppurate in two or three weeks, and continued until her death on April 25, 1882. She left my service on account of erysipelas, with which she was attacked, and I did not see her again until a few days before she died. Dr. Magnin, of Bellevue Hospital, and Dr. Healy, of Hart's Island, kindly took charge of the patient for me.

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<sup>1</sup> Notes by kindness of Dr. Cramer, House Surgeon Mt. Sinai Hospital.

On April 26, 1882, I presented the artery before the New York Pathological Society, together with the carotids of a horse and a large grayhound,<sup>1</sup> which had been tied with nerve ligatures. The artery was completely occluded and its continuity unbroken. There was a depressed ring, scarcely appreciable, at the point where the ligature had constricted it. No result could have been more perfect than was accomplished by this ligature. It was as large as the median or ulnar nerve in the humeral region of a full-sized man, and was taken from the calf, and kept in five per cent. carbolic water for twenty-four hours before using.

The advocates of the tape-like, carbolized, animal ligature argue that it is safer than the smaller, harder, violin-string sheep-gut ligature (or any other thread), since: 1. It includes in its pressure a larger area of artery, and hence does not cut into the coats of the vessel, either by the force of the ligature when it is tied, or by the subsequent expansion and friction of the arterial walls against the narrow thread. 2. It does not divide any tunic of the artery, simply jamming and wrinkling it, bruising the intima, causing a slight inflammation and proliferation of the endothelia, which result in permanent occlusion.

Howard, in his Prize Essay says: "The force with which the ligature is applied strangulates the *vasa vasorum*, and thus compels the part included in the loop and immediately beyond it to die. Through this thin remaining coat the ligature rapidly ulcerates. Roux, Hodgson, Brodie, Thompson, Erichsen, Velpeau, Nélaton, Guthrie, Gross, and others, agree that the portion of the artery thus strangulated is killed and must slough away."

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<sup>1</sup> The carotid of the dog was also occluded. The ligature had slipped from its place on the horse's artery and it was permeable. There was a roughened surface at the point of deligation, due to proliferation of the endothelia. At the time of the operation the animal was plunging violently, and I do not think that I did the deligation well. Both autopsies were made in the fifth week, and the nerves had been completely absorbed. I wish to thank Dr. J. W. Baker, U. S. N., and Dr. Macgillicuddy for valuable assistance.

My own operations most *positively prove that this conclusion is not correct*, for the continuity of the artery was unbroken, although the ligature had completely occluded it and had disappeared by absorption.

Nerve tissue seems to me to be especially suitable as a ligature.<sup>1</sup> It is easily obtained, is very strong by virtue of its neurilemma, and is soft and cushioned, since its cylinders of neurilemma are filled with the white substance of Schwann.

The following list contains, as far as I am able to learn, all the cases in which the tape-like animal ligatures have been used upon arteries in their continuity.

#### CLASS A.—*Ox-aorta.*

Four double distal operations upon the right carotids and the third portions of the right subclavians. In one of these (Mr. Barwell's), at the death of the patient, sixteen and a half months after the operation, the subclavian was occluded; the carotid was only partially so. This was Mr. Barwell's first case, and he says it was tied too loosely. In another case, by this surgeon, the patient died thirty hours after the operation, from pulmonary complications.

One right subclavian (recently tied). Result not yet known.

One right carotid. Successful.

One left carotid. Successful.

Two external iliacs. Successful.

Four superficial femorals. Successful.

One popliteal, above and below a traumatic aneurism. Successful.

One brachial. Successful. In this case catgut had been used and had failed, necessitating the use of ox-aorta for the arrest of secondary hemorrhage.

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<sup>1</sup> In order to prevent any possibility of slipping, I tie the two ends of the knot together with a catgut.

This gives a total of 19 cases.

CLASS B.—*Kangaroo tendon.*

1. Double distal, Pollock.<sup>1</sup> Right subclavian, third division. Right carotid.

In tying the second loop of the knot upon the carotid, the tendon broke, but the first knot held the artery occluded. Fearing it might be insecure, a catgut was applied to this vessel. Patient died ten days after operation. Both arteries were found occluded after death. Ligatures undergoing process of absorption.

2. External iliac. Successful.

CLASS C.—*Nerve ligatures.*

Right carotid. Successful.

Omitting Mr. Barwell's case, which died thirty hours after the operation; the right subclavian, the result of which is not yet obtained; and the carotid in Mr. Pollock's case, in which the tendon broke and was reinforced by catgut, there are twenty large arteries which have been tied with tape-like animal ligatures. In no case has hemorrhage occurred, and all have resulted favorably, except one, in which the operator purposely did not draw the ligature tightly.

I conclude this article with the following extract from a letter recently received from Mr. Barwell.

"I have no doubt you will find nerves, or any other fresh animal substance, act well. We know that all the soft connective tissues, as tendon and cellular membrane, possess this faculty of becoming absorbed or incorporated in the tissues, provided they are perfectly fresh. The extensor tendons of a kangaroo's tail, tendon from the whale, decalcified bones from quadrupeds and birds, have all been introduced into the human body, and have thus acted in the

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<sup>1</sup> *Medico-Chir. Trans.*, vol. lxiv, p. 231.

tissues. In the selection of a ligature the important points seem to me to be that the substance should be absolutely free from all taint of decomposition, and that it should be cleansed from any accidental impurity by a short immersion in some antiseptic fluid. If these essentials be secured, the choice of a particular substance is immaterial; sufficient strength, persistency to keep a safe knot, as also facility of acquisition, are the main considerations."

## THERAPEUTIC CONTRIBUTIONS.

### V.

ON THE EFFICIENT DOSAGE OF CERTAIN REMEDIES USED IN THE  
TREATMENT OF NERVOUS DISEASES.\*

BY E. C. SEGUIN, M.D.

#### II.—CRYSTALLIZED ACONITIA OF DUQUESNEL.

(*Aconitia*.—U. S. P.)

Doses as given by authorities on *materia medica* and *therapeutics*:

STILLÉ and MAISCH, *National Dispensatory* (1879), p. 101.<sup>1</sup> Primary dose  $\frac{1}{250}$  grain two or three times a day. It is recommended in doses of  $\frac{1}{25}$  grain.

WOOD, *Therapeutics* (1880), p. 180, makes the truly astonishing statement that: "The alkaloid is officinal, but, on account of its intense activity, should not be given internally."

This was printed more than a year after the publication of the New York Therapeutical Society's report on *aconitnia* in the *New York Medical Journal* for 1878.

\* This article is a continuation of one which appeared with the same title in the April number of this journal, page 177.

<sup>1</sup> In this paragraph occur several serious misprints. The dose of  $\frac{1}{25}$  grain is rendered as gm. 0.005, which is really  $\frac{1}{2}$  grain, and might prove fatal. In speaking of external applications, 2 and 5 grains are rendered as gm. 0.133 and 0.333, which are correct figures; but immediately after it we find one (1) grain rendered as gm. 0.666, in reality nearly eleven (11) grains.

BARTHOLOW, *Materia Medica* (1880), p. 44, simply quotes the New York Therapeutical Society's formula. No personal statement as to doses.

RICE. *Posological Tables* (1879), p. 5: "Aconitia; aconitine. Alkaloid from aconite. The commercial product is an impure mixture of alkaloids. The dose is  $\frac{1}{160}$  to  $\frac{1}{130}$  grain, increased with caution. Chiefly externally."

NOTHNAGEL and ROSSBACH. *Arzneimittellehre* (1878), p. 721. Aconitia is little employed internally. Dose, gm. 0.004 or  $\frac{1}{16}$  grain, and the daily quantity as gm. 0.03 or  $\frac{1}{2}$  grain.

This cannot refer to Duquesnel's aconitia. It might be a safe guide for giving Merck's aconitia, which is very impure and of doubtful efficacy.

GUBLER. *Leçons de Thérapeutique* (1877), pp. 147-8. Prof. Gubler may be considered as the introducer of Duquesnel's aconitia. In articles, besides in this book, he was the first to indicate its wonderful efficacy in neuralgia, particularly trigeminal neuralgia.

He recommends gm. 0.0005 ( $\frac{1}{130}$  grain), or less at first; gradually increased to gm. 0.002—0.004—0.005 ( $\frac{1}{30}$ ,  $\frac{1}{16}$ ,  $\frac{1}{8}$  grain).

Doses recommended by clinicians.

As Duquesnel's aconitia has been known so few years, and has been in use less than four years in this country, it is not singular that our principal text-books do not speak of it. Still one is surprised to find that Prof. Flint in the last edition of his "Practice," dated 1881, does not refer to aconitia among the remedies which may cure neuralgia.

HAMMOND, *Diseases of the Nervous System* (1881), pp. 857-8, speaking of the treatment of neuralgia, recommends Duquesnel's aconitia in doses of  $\frac{1}{120}$  grain, gradually increased to  $\frac{1}{8}$  grain if necessary, till relief be obtained, or till the characteristic peripheral numbness occurs.

Personal experience. Influenced by Prof. Gubler's article and by his book, I began using the aconitia of Duquesnel in the winter of 1877-8, with most gratifying results. More of the drug was imported, and in a few months several of my friends were trying the remedy—among them I may name Dr. McBride and Dr. Andrew H. Smith.

At a meeting of the Therapeutical Society of New York held October 11, 1878, I presented the report of the Committee on Neurotics of that Society upon the use of this aconitia. We reported ten cases cured or relieved. This report will be found in the *New York Medical Journal* for December, 1878.

Since that time aconitia has been used by many physicians in numerous cases of trigeminal neuralgia, with very favorable results. A large proportion of cases have been cured, and some very ancient cases (8 to 12 years) greatly relieved by the medicine. A few cases only have been uninfluenced.

In the last two years the alkaloid has been offered in pillular form by several reliable drug-firms, and I can testify to the potency and reliability of Caswell & Hazard's tablets, and of Schieffelin's pills. These firms furnish doses of  $\frac{1}{200}$  grain and of  $\frac{1}{100}$  grain.

In my first use of aconitia I employed a solution made by the late Dr. William Neergaard, the only pharmacist who then (1877-8) held a sample of Duquesnel's preparation. My formula was :

R			
Aconitiae (Duquesnel's), gr.	$\frac{1}{10}$		
Glycerinæ,			
Alcoholis,      aa		3 i	
Aquaæ menth. pip., ad			3 ii

Each teaspoonful (estimating seven teaspoonsfuls to the ounce) contained about  $\frac{1}{140}$  grain. This dose was to be given two, three, or more times a day, on an empty stom-

ach, till the pain ceased or the physiological symptom—numbness—was produced. As my subject to-day is not clinical therapeutics as much as posology, I pass by many interesting facts about the use of aconitum and omit all cases.

The remark which I have already made about the necessity of giving small doses of potent drugs to a patient whom we see for the first time, and of estimating his susceptibility, applies with especial force to aconitum. Bearing this in mind and carrying it into practice we may be very bold, almost rash, later on, without running real danger.

Those of us who introduced aconitum in 1878 soon discovered that some persons, females especially, were powerfully affected by minute doses. Dr. A. H. Smith reported a case to our Committee in which a lady was distressed by  $\frac{1}{400}$  grain, and I myself, while in a reduced state of health and suffering severe trigeminal pain, was severely benumbed by  $\frac{1}{200}$  grain (though long afterward, when quite well, it required two doses of  $\frac{1}{100}$  grain to produce nearly similar effects).

It is well, consequently, to give debilitated, susceptible, and female patients, doses of  $\frac{1}{250}$  or  $\frac{1}{200}$  grain (from gm. 0.0002 to gm. 0.0003) to begin with. These facts have induced the Messrs. Schieffelin & Co. to cease making pills of  $\frac{1}{100}$  grain, and to furnish only the  $\frac{1}{200}$  grain which can be repeated at will. Messrs. Caswell & Hazard still furnish both doses in the shape of soluble tablets.

In a case of neuralgia, after a day's testing with minute doses, if I find no undue susceptibility to the drug I give it freely— $\frac{1}{100}$  grain every 3 or 4 hours until distinct numbness and coldness (subjective coldness) be felt in the limbs and face. Then a longer interval may be allowed before giving another dose. Some subjects will take 3 or 4 tablets of  $\frac{1}{100}$  grain, each day, and be in a constant state of numbness without harm, and often with curative effect.

In some of my cases of chronic epileptiform neuralgia I have kept patients under the influence of the drug for days and weeks,—and have seen no evidence of cumulative effects.

As a rule, in testing a man of average physical development and not reduced by disease I at once start with doses of  $\frac{1}{100}$  grain (gm. 0.0006).

As regards maximal doses, I may state that in certain cases of posterior spinal sclerosis with severe fulgurating pains I have given from 4 to 8 doses of  $\frac{1}{100}$  grain each in 24 hours, producing in some cases faintness, sickness, and a considerable prostration. I might add that this form of nerve pain has never been relieved by aconitum, and that with hardly an exception, all the tabetic patients I have experimented on have not shown any trace of the numbness which is *the sign of aconitum effect* in healthy persons.

As a rule, the pain of trigeminal neuralgia ceases when the physiological effects of the drug are manifest. I do not pretend, and Prof. Gubler did not claim, that aconitum is a certain or specific remedy against trigeminal neuralgia, but it certainly is the best of all our present therapeutic resources against this terrible disease. Of course in certain cases, special etiological factors must be considered, and other treatment given besides the aconitum: for example, in clearly malarial neuralgia, and in syphilitic neuralgia, or in the (rare) neuralgia from bad teeth.

### III.—PHOSPHORUS AND PHOSPHIDE OF ZINC.<sup>1</sup>

(*Phosphorus*.—U. S. P.)

Doses given by authorities on *materia medica* and *therapeutics*:

STILLÉ and MAISCH. *National Dispensatory* (1879), p.

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<sup>1</sup> The equivalent of zinc phosphide ( $Zn_3 P_2$ ) is  $195.6 + 62 = 247.6$ . Consequently one part of the phosphide contains 25 % (about) of phosphorus.

1072. These authors, apparently wholly relying upon Gubler and Thompson, state that the dose varies from  $\frac{1}{30}$  to  $\frac{1}{4}$  grain. They say: "Those who have most advocated its use recommend that a first dose of one eighteenth of a grain (gm. 0.003) should be repeated every four hours till six doses are taken. If then no improvement (in neuralgia) have occurred, the dose should be increased to one twelfth of a grain (gm. 0.005), and repeated in the same manner as before."

They do not, however, mention Thompson's alcoholic solution of phosphorus.

Zinc phosphide (p. 1546) in doses of  $\frac{1}{16}$  to  $\frac{1}{8}$  grain, and even  $\frac{1}{4}$  grain.

STILLÉ. Therapeutics (1874), vol. i, p. 800. "Moderate doses of  $\frac{1}{40}$  to  $\frac{1}{4}$  grain." (Phosphorus.)

WOOD. Therapeutics (1880) p. 113. Recommends a mixture containing oleum phosphoratum, each dose to contain from  $\frac{1}{30}$  to  $\frac{1}{15}$  grain; or of a chloroformic solution in a mixture,  $\frac{1}{6}$  grain.

The dose of zinc phosphide he gives as  $\frac{1}{100}$  to  $\frac{1}{6}$  grain, which is in strange contradiction to his full doses of phosphorus.

BARTHOLOW. Materia Medica (1880), p. 96.

Dose of oleum phosphoratum, U. S. P., 5 to 10 drops (equal to  $\frac{1}{24}$  or  $\frac{1}{12}$  grain, as each ml. of Ol. P. = gr.  $\frac{1}{20}$  of P.).

Quotes Radcliffe's formula for pil. phosphori,  $\frac{3}{100}$  grain in each pill. Also quotes Thompson's tinctura phosphori in doses equivalent to  $\frac{1}{6}$  and  $\frac{1}{2}$  grain.

The dose of phosphide of zinc is  $\frac{1}{2}$  to  $\frac{1}{4}$  grain.

RICE. Posological Tables (1879). Oleum phosphoratum (p. 54). No dose given. Recommends Dr. Squibb's solution: Phosphorus, 1 part; cod-liver oil, 99 parts.

"Phosphorus,  $\frac{1}{100}$  to  $\frac{1}{20}$  grain, increased with caution."

NOTHNAGEL and ROSSBACH. *Arzneimittellehre* (1878), p. 200. Dose from  $\frac{1}{63}$  to  $\frac{1}{12}$  grain (gm. 0.001—0.005).

GUBLER. *Leçons de Thérapeutique* (1877), pp. 236-7. Dose, gm. 0.001 ( $\frac{1}{63}$  grain) in granules; from 2 to 10 a day.

Praises the oleum phosphoratum in capsules.

Zinc phosphide, from  $\frac{1}{6}$  to 1 grain (gm. 0.01—0.05) *per diem*; he rather depreciates its virtues.

Doses as given by authorities on clinical medicine:

HAMMOND. *Diseases of the Nervous System* (1881), p. 69. Speaking of cerebral congestion, he says that the oleum phosphoratum may be given in a mixture in doses of 5 drops (or about  $\frac{1}{25}$  grain of phosphorus).

Zinc phosphide, the formula of which he gives as  $Zn_3 P.$ , and estimates as containing  $\frac{1}{4}$  of phosphorus, he recommends in  $\frac{1}{10}$  grain dose, in pill form (this gives  $\frac{1}{70}$  grain of phosphorus); or the phosphoretted resin may be used to make pills, each containing  $\frac{1}{70}$  grain of phosphorus.

FLINT. *Practice of Medicine* (1881), p. 797. Merely names phosphorus as a remedy for neuralgia; gives no doses or estimate of its value.

ANSTIE. *On Neuralgia* (1871), p. 180. States that he has used the phosphuretted oil and pills of phosphorus (Dr. Radcliffe's), containing  $\frac{1}{30}$  grain, three times a day. He does not estimate it as specially useful. (This was written before the publication of Thompson's work.)

J. ASHBURTON THOMPSON. *Free Phosphorus in Medicine*, London (1874), p. 190: "The chief precaution to be observed in the treatment of neuralgia with free phosphorus

\* \* \* is to administer a full dose of the remedy in the first place."

" \* \* \* unless half a grain or more be given in the course of each twenty-four hours, frequent failures, or only partial successes in treatment will be met with." "But the remedy must be given in not less than this dose, *i. e.*, one

twelfth of a grain repeated every four hours, from the beginning of treatment."

Page 191. He admits the utility of the alcoholic and ethereal solutions, reduced phosphorus, and even zinc phosphide, but he has had the best results from one twelfth of a grain of phosphorus dissolved in cod-liver oil, every four hours.

Thompson has more recently furnished the following formula for the preparation of a solution of phosphorus, which is not unpalatable to most patients.

Take of

Phosphorus,	gr. i
Absolute alcohol,	3 v
<i>Dissolve with heat.</i>	
Glycerine,	3 xii
Alcohol,	3.ii
Essence of peppermint,	3 ii.

Mix the two solutions, which make nearly 3 xx;  $3 \text{ i} = \frac{1}{20}$  grain. This should be given without water.

Personal experience. Very soon after the appearance of Dr. Thompson's article, I caused this solution of phosphorus to be made by Mr. F. Haas, by Caswell, Hazard, & Co., and by the late Dr. Neergaard, and used it a great deal. A weaker preparation or imitation, under the name of elixir of phosphorus, one teaspoonful of which contains  $\frac{1}{40}$  grain, is also sold, but I prefer the stronger form, and write for *solutio phosphori* (Thompson).

I have employed this solution with the greatest success in trigeminal neuralgia, and with some success in other neuralgias—following Thompson's plan of giving full doses, usually 1 teaspoonful (about  $\frac{1}{16}$  grain, if we estimate a teaspoonful to be a little over 3 i), every 3 or 4 hours. I have known a severe facial neuralgia (not chronic epileptiform neuralgia) cured in two days, and even in 24 hours; several cases in a week.

In conditions of nervous prostration, cerebral anaemia, in-

cipient cortical degeneration (dementia), in melancholia, I have been much pleased with a combination of Thompson's solution and cod-liver oil in the proportions of 1:6 or 1:4, a tablespoonful of the mixture being given after each meal.

In other cases I have had an extemporaneous mixture made and given two or three times a day: Thompson's solution, 1 teaspoonful; sherry, 2 tablespoonfuls; cod-liver oil, from 1 to 2 tablespoonfuls; and the yolk of one egg, thoroughly beaten and mixed, with the addition of a little extra oil of peppermint. This is well received by most patients, and constitutes a most valuable tonic.

The phosphide of zinc in doses of  $\frac{1}{6}$  to  $\frac{1}{4}$  grain (gm. 0.01 to 0.0125) combined with nux vomica or with belladonna, according to indications, has seemed of some efficacy in the treatment of posterior spinal sclerosis, of cerebral anaemia, of nervous prostration ("neurasthenia"), and of incipient dementia.

With pills of pure phosphorus I have had little experience. The pills offered by most of our manufacturing drug-concerns are of too small a dosage. As may be seen from the citations made, and from my own experience with other preparations, the giving of  $\frac{1}{100}$  grain (gm. 0.0006), or even of  $\frac{1}{50}$  grain (gm. 0.0012), of phosphorus is of probably very little use. From  $\frac{1}{30}$  to  $\frac{1}{10}$  grain (gm. 0.002—0.006) should be administered three times a day, with, of course, due watchfulness for signs of gastric irritation.

#### IV.—CRYSTALLIZED NITRATE OF SILVER.

(*Argenti nitras.*—U.S. P.)

Doses as given by authorities on *materia medica* and *therapeutics*:

STILLÉ and MAISCH. National Dispensatory (1879), p. 237.

Dose from  $\frac{1}{6}$  to  $\frac{1}{4}$  grain, three times a day. Doses of  $\frac{1}{2}$

grain occasion no special symptoms, but larger quantities are apt to cause gastric heat, pain, and nausea.

STILLÉ. Therapeutics (1874), i, p. 367, *et seq.*

Quoting authors upon diseases of the nervous system he refers to doses varying from  $\frac{1}{4}$  to  $\frac{1}{2}$  grain, three times a day.

WOOD. Therapeutics (1880), pp. 51-3.

Dose from  $\frac{1}{4}$  to  $\frac{1}{2}$  grain, in pill form, given upon an empty stomach, when it is desired to affect the stomach, and after meals, when the constitutional effects of the drug are desired.

"When given for a chronic disease, its administration should be suspended for one week, at the end of every third week, and its employment should not extend over a longer time than three months, without a protracted intermission."

BARTHOLOW. Therapeutics (1880), pp. 213-5.

Gives elaborate directions and formulas for its use in various visceral affections, dyspepsias, gastritis, colitis, etc., but hardly refers to its use in nervous diseases, and does not recommend it.

RICE. Posological Tables (1879).

Dose  $\frac{1}{4}$  to 2 grains: increased with caution.

NOTHNAGEL and ROSSBACH. Arzneimittellehre (1878), p. 113.

In pill-form, gm. 0.005—0.03 ( $\frac{1}{12}$  to  $\frac{1}{2}$  grain).

GUBLER. Leçons de Thérapeutique (1877), p. 579.

Thinks that no good effects are to be expected from its internal use, and gives no doses.

Doses as given by authorities on clinical medicine:

The older English physicians, Sims, Wilson, Harrison, and Roget, quoted by STILLÉ, Therapeutics, i, p. 367, gave doses of one, two, three, and even six grains, three times a day, for epilepsy. It is not now employed for this disease, I believe.

FLINT. Practice of Medicine (1881), p. 476.

Speaking of locomotor ataxia, he recommends giving from  $\frac{1}{8}$  to  $\frac{1}{4}$  grain, three times a day, for several weeks; then suspending its use for a while.

HAMMOND. Diseases of the Nervous System (1881), p. 633.

In the treatment of locomotor ataxia, merely mentions dose of  $\frac{1}{4}$  grain three times a day; gives no details, and does not seem to attach any value to the drug.

WUNDERLICH, *Archiv der Heilkunde*, 1861, ii, p. 193 (cited by STILLÉ, p. 368), gave  $\frac{1}{8}$  grain twice and thrice a day; quoted by TOPINARD, he gave  $\frac{1}{8}$  grain three or four times a day: for locomotor ataxia.

BOUCHUT (cited by STILLÉ, pp. 368-9), *Bull. de Thérap.*, lxiv, p. 57, gave to a child 5 years old, with paraplegia,  $\frac{1}{10}$  grain twice a day. To adults for paralysis from  $\frac{2}{3}$  to 1 grain a day.

TOPINARD, *De l'ataxie locomotrice*, Paris, 1864, pp. 435-468, gives a full account of the attempts to cure sclerosis of the posterior columns by silver; relates several cases of his own, and concludes that the drug is usually useless in locomotor ataxia; he gave from  $\frac{1}{8}$  to  $1\frac{1}{2}$  grains *per diem*.

He gives the following doses as prescribed by several well-known physicians.

CHARCOT and VULPIAN in 1862 gave doses of  $\frac{1}{8}$ ,  $\frac{1}{4}$ ,  $\frac{1}{2}$  grain three times a day. Later CHARCOT has given as high as  $1\frac{1}{2}$  grains in a day.

PIDOUX,  $1\frac{1}{2}$  grains *per diem*.

GUBLER and BEAU,  $1\frac{1}{2}$  gr. *per diem*.

HILLAIRET,  $2\frac{1}{2}$  gr. *per diem*.

W. ERB. Ziemssen's Cyclopaedia, Am. ed., vol. xiii. On Diseases of the Spinal Cord, pp. 614-5. Recommends from  $\frac{1}{8}$  to  $\frac{1}{4}$  grain three times a day, or from 1 to  $1\frac{1}{2}$  grains *per diem*, until 120 or 150 grains have been consumed. He has a high opinion of the medicine, for he says, p. 614:

"Among the *internal remedies* for tabes, nitrate of silver undoubtedly stands first, as it can show quite undoubted results."

Personal experience. I have employed nitrate of silver extensively in the treatment of locomotor ataxia, and am almost disposed to agree with Erb. I can most positively assert that in quite a number of my cases the course of the disease has been materially checked, and in many others repeated periods of relief secured by nitrate of silver.

I have also given it in various forms of subacute and chronic myelitis, but with less definite results ; though my impression of its action in these cases is favorable.

I seldom prescribe less than  $\frac{1}{4}$  grain (gm. 0.0125) of silver at a dose, and usually give  $\frac{1}{2}$  grain (gm. 0.03). The remedy is made up in a pill with an indifferent extract (*taraxacum*), or with extract of *nux vomica*, or with extract of *bella-donna*, according to the indications of the case, whether for spinal stimulation or for sedation.

I always give the pill before meals, three times a day, and occasionally administer a fourth pill at bedtime. A course of silver, with me, usually lasts two months, which, at the rate of  $1\frac{1}{2}$  grains (gm. 0.10) a day, would give 90 grains (gm. 6.)—a perfectly safe quantity as regards danger of discoloration of the skin. After an interval of two or three months I often give another, shorter course, and so on.

None of my patients has as yet shown discoloration (*argyria*), and I have seldom been annoyed by the occurrence of gastric and intestinal irritation. *Albuminuria* I have never seen.

## EDITORIAL DEPARTMENT.

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### A CONTRIBUTION IN RE GUITEAU.\*

BY HENRY P. STEARNS, M.D.,

SUPERINTENDENT OF THE RETREAT FOR THE INSANE, HARTFORD, CONN.

"When we in our viciousness grow hard—  
O misery on 't!—the wise gods seal our eyes ;  
In our own filth drop our clear judgments ; make us  
Adore our errors ; laugh at 's while we strut  
To our confusion."

*Antony and Cleopatra*, act 3d.

**F**IRST. We know nothing objectively of mind except as it is manifested through the brain.

Second. The brains of no two persons are, in all respects, arranged or constituted exactly alike, and, consequently, manifestations of mind as to memory, judgment, and ability to reason, will differ, in some degree, in all persons.

Third. Pathological changes in the brain and other organs of the body produce, sooner or later, *impairment* and *change* in the character of the functions of these organs, and we determine the character and probable extent of disease by studying the nature of the change.

Fourth. It, therefore, follows that in studying mind in its relation to disease, we should do so primarily by comparing

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\* Read before the New England Psychological Society, at its regular meeting, held in Boston, April 11, 1882.

its character as presented at the time being with that known to have existed during some definite period of its former history, and also with reference to the element of duration.

As this, however, is not always practicable, on account of our imperfect knowledge of the character and mental history of the individual, it becomes necessary, in such a case, to form an opinion on other data; and, as there exists a sort of common standard of mental action, a comparison may be made with this, and an opinion formed as to quality of mind in relation to health and disease, in this manner. In following this second method it becomes necessary to bear in mind always that the standard of comparison is one more or less variable in its character, and hence that in doubtful cases it is of the utmost importance to ally it with the first method of investigation.

In this paper it is proposed to study the evidence in the case of Guiteau in these two ways, long recognized as affording the most reliable method of examination, viz.: 1st, in relation to *change of character*, and 2d, in relation to *the common standard of mental activity*, and close with a few statements as to the results of personal examination.<sup>1</sup>

#### I.—*In Relation to Change of Character.*

It appears that from his youth Guiteau was headstrong, wilful, and impatient even of parental authority, and at the age of 14 years he had a physical contest with his father; that he was unwilling to follow advice, was inconstant of purpose, and at the age of 16 was immoral, and that at 19, in the opinion of his father, he went to the Oneida community that he might the more easily gratify his sexual desires; that during five or six years of that portion of life when character is more rapidly forming, he resided in a community whose members profess to believe that a violation of those prin-

<sup>1</sup> As the family history and the evidence in the case are so familiar to my readers, I shall not encumber the text by reproducing them in this paper.

ciples of morality which are recognized as binding even among savage nations, is right, and hold it as a part of their religious creed and practice ; and some of whose members, while claiming to be Christians, and even inspired, yet daily tread the precepts of Christianity into the mire of sensuality, and seek to destroy those distinctions between right and wrong by which alone society is made possible.

It is in evidence that his practice of law consisted mainly in making collections, and in evading his just obligations toward those who employed him ; that he was in the habit of visiting those in prison and obtaining money without accomplishing any thing for their relief ; that he used various artifices, under the forms of law, to obtain possession of the property of others.

It is in evidence that he defrauded nearly everybody in reference to his living for years ; that he wandered about the country regardless of social or family obligations ; that he disliked his father, and did not visit him for years, and rarely visited other members of his family ; that he was more than once in prison for criminal acts, and during months in daily contact with criminals.

It appears that while professing to be a Christian and anxious to save souls, he seemed to be greatly more anxious to evade payments of obligations ; that he avoided lecturing in States where he could not escape fines for violating law ; that he made his Christian profession a cloak with which to shield himself from the consequences of violated law, and finally gave up his lecturing because "theology did not pay."

It is in evidence that he was selfish and self-seeking, and anxious for notoriety in some form or other ; that when reproved by his only brother for leading such a mode of life as he was accustomed to, for making false representations, and obtaining money and trust by false pretences, he replied

that he would not listen to such reproof longer, that he was a fighting man and would retaliate; that on another occasion, for a similar reason, he did strike his brother in the face; and further, that for years he had gone down step by step, finding it more and more difficult to obtain the means of subsistence by such methods as he had employed for years.

It is also in evidence that there had existed and been manifest from the period of his boyhood, a boundless egotism, and a consequent credulousness in reference to every thing pertaining to himself; that he had a judgment far less reliable than is common, both as to the conduct and motives of himself and other persons; that his ambition by no measure compared with his education and lack of persistent application, so that he constantly exhibited a character both unbalanced in its faculties, and inharmoniously developed.

Constant disappointment and failure in plans of life usually have a profoundly depressing effect upon minds as ordinarily constituted, but upon such as above described, they have comparatively little. The egotistical element rises above and triumphs over such misfortunes as would crush more ordinary ones, and they pass harmlessly by, while a disregard of moral obligations tends more surely to demoralize and degrade them.

Referring now to the mental characteristics of Guiteau as presented at the time of the trial, I think we may say that they are such as are foreshadowed by his previous history; that he was vastly egotistical and self-credulous, that he was selfish and self-seeking, that he exhibited malignity of character in hatred of his brother and other members of his family, that he was oblivious of moral obligation, and more so now than he was twenty years ago.

Has there, however, come in his character any other than such change as we should expect from development of

character under such surroundings and conditions of life? Bearing in mind that as years pass on all character changes in some measure, and in accordance with circumstances, education, and the efforts of will made by its possessor; that it becomes better or worse, stronger toward right and truth, or weaker toward evil, year by year, in proportion as temptations are resisted or yielded to, I am unable to conclude that any other change has come in the case of Guiteau than such as was the legitimate outgrowth of his mental constitution and his mode of life.

I certainly find no evidence that at any particular period of his life there was any such marked change as evinced a pathological condition of his brain. To assume that such a change began, or was indicated by his striking his father when 14 years of age, or by giving up his studies and going to the Oneida Community when 19, is simply begging the question. These acts by themselves are no evidence of insanity either in him or in the many others who have acted in a similar manner. Every one has to decide for himself what course in life he will pursue, and not a few have begun a course of study and relinquished it. Guiteau did so, and thought he would prefer to follow such a course of life as he thought existed in the Oneida Community. In adopting this course there is no evidence that he acted hastily, confusedly, or unadvisedly, and there is no more reason for supposing him insane on this account than for supposing others are who follow an unwise or a fanatical course in life. There is no reason to suppose that any of the Community thought him insane when he came there, or that he acted in a manner different from others who came. There is no ground for assuming that his conduct while there was other than would be likely to come from one who had any right views as to purity and virtue left, or any aspirations toward a better or different mode of life.

The very fact that he became uneasy, restless, and dissatisfied with his life there and anxious to get away evinced healthy rather than diseased mind. The fact that after he had escaped, and had come in some measure to realize his great injury, he felt indignant and revengeful, and tried to obtain compensation for loss of time, was in keeping with good-sense.

That after being buried five or six years in the moral and intellectual darkness of such a community, he should, with his innate ambition, on coming into the light and freedom of ordinary life, largely overestimate his own ability; that he should misjudge as to the public requirements in the way of religious reading, imagining that society in general was much like the little one in which he had lived; that he should indulge in projects impracticable, so far as he was concerned, is perfectly natural, and was one of the results of narrow experience and ignorance of the world at large upon such a mind as his. Moreover, it should be borne in mind that thousands, who have had higher advantages than Guiteau, have overestimated their own ability; have had visionary projects, one after another, all their lives, and yet have not therefore been insane. Guiteau's egotism was certainly remarkable, and yet his own sister and brother seemed to have almost as much as he did; and they evidently greatly enjoyed being the observed of all observers, even under the very painful surroundings of the trial; they were both ready to, and on several occasions did, attempt to put questions to the witnesses, and were prevented from doing so only by the repeated orders of the Court. I, therefore, conclude that this desire for notoriety, instead of being an evidence of any morbid change in the brain, is a trait of character native in the Guiteau family, and that Charles Guiteau has it only a little more pronounced than the other members.

Besides, it is important to remember that love of notoriety, ambition, a weak judgment, immorality of any kind, an inability to make a connected or logical speech of ten or twenty sentences, a readiness to compliment the court, the jury, or witnesses, while one thinks there is something to be gained by so doing; or a readiness to denounce them when there is no longer any thing to be gained from them; a failure to appreciate the proprieties of the court-room, or one's obligations toward friends who are trying to do their best, or any other peculiarity, or eccentricity, is of little moment except when considered in relation to *change in quality of mental operations.*

The important question is, whether, in these exhibitions of mind, there was any confusion of thought, incoherence in language, dulness of comprehension, failure in understanding, inability to know the bearing or relations of what he did and said, and of what was done and said by lawyers and witnesses, or, in any other way, exhibition of *intellectual impairment?* On the contrary, he understood so fully and so well the bearing of all that was said and done, and perceived the weak points in his case so clearly, that he again and again protested and begged that he might have more able counsel. While glancing over the newspaper he was alert and always ready with some statement, or joke, or sharp rejoinder, with the view of weakening an unfavorable impression. I think he did not denounce a single person except after he had concluded that he could get no more out of him to his benefit, or when he thought he had compromised his case. He showed that he fully realized that the issue of his case turned on the one possibility of his being able to convince the jury that he was insane at the moment of shooting. In fact, I fail to find any evidence that at any former period of his history his intellect was more strong and clear in its operations than it was during

his preparations for shooting the President, and during the trial.

Insanity becomes an exceedingly hazy and uncertain quantity when made to depend on any kind of eccentricity, or conduct, except such as evinces *failure, impairment, and change of intellect.*

## II.—*In Relation to the Common Standard of Mental Activity.*

I now proceed to the second method of examination, viz., *a comparison with the common standard of mental activity.* In this proceeding it becomes necessary to select some well-known transaction, or period of mental history, by means of which to institute a comparison, and which will afford scope for the exhibition of such quality of mind as pertains to the actor. I propose, therefore, in this case to select the shooting of the President, which, perhaps, will give as severe a test of mental action as any could, and which will be better for our purpose than any other, as we are able to trace it quite clearly and fully from the beginning to the end. Our examination of this will relate, first to *plan*, and second to *motive*.

### First.—As to *plan*.

Let us suppose that for any reason Guiteau had fully determined in his own mind to remove the President: the first question that would arise would be as to how it could be accomplished. A plan becomes necessary, and a moment's consideration shows that very great difficulties in the way of accomplishing such an act and escaping its immediate consequences present themselves.

1. The President could not be visited by Guiteau in the White House, as he had already been refused an audience many times.

2. The President rarely went abroad in the city alone, or

was present at any such time or place that he could be shot, except with great danger to other persons, and at the greatest immediate peril to the criminal.

3. Guiteau was unable to provide himself with the means of either flight or safety for any length of time; he had neither money nor friends of influence, nor ability to secure a place of refuge; he could not divulge his purpose to any one, lest, as he himself said, he should at once be locked up. Moreover, if these difficulties could have been overcome, the purposes he had in view in reference to any future for himself could not have been accomplished by such methods. The largest results which could have come to him from any immediate security would be a short delay in the execution of justice. If, therefore, the President is to be removed, it must be done in the broad light of day, and only at the greatest peril to Guiteau.

Under such circumstances and almost insurmountable difficulties it becomes necessary to form a plan, and one far-reaching in its bearings, if there is to remain the shadow of a hope of life afterward. How far-reaching his plan actually was and how much it involved, appears from the history, thus far, of its execution.

Fortunately, or unfortunately for Guiteau, the basis for such a plan was ready at his hand. The President was at variance with a portion of his party; the interest of a large portion of the public in the solution of the difficulty was intense. The newspapers were full of the possibilities of the situation and the probabilities of its results. Now the plan of Guiteau embraced the following points.

1. The shooting the President under such circumstances as to create the largest possible confusion in the minds of all who should be in the immediate vicinity.

2. Escaping the hands of the mob by securing the means of conveyance at once to the prison of the District,

and by asking that the prison be guarded by a detachment of the army to prevent immediate violence, thus showing that he fully considered the effect upon the public mind of such an act.

3. The allaying of all party strife in the midst of such a profound public calamity, and a consequent union of factions, and a harmony of action under the new administration.

4. The placing in power and positions a large number of persons who, under other circumstances, would not have been so benefited, and a consequent obligation arising from such personal advantages growing out of his act in removing the President.

5. A foreseeing that even such results in the way of allaying party strife, and securing union of feeling and sentiment, and individual emolument, would be inadequate to protect him from the punishment of his crime, and, therefore, that something additional must be pleaded in extenuation. This was devised from an experience which had for years been familiar to the mind of Guiteau, viz.: an *inspiration from Deity* to do the act.

On this part of the plan all the other details hinged. Except for this, the others would be of comparatively little use, and for this all the other portions were arranged: To escape the mob, get behind the prison walls, and wait for the results which were sure to come in change of administration and office, and in the public mind, and gain an opportunity to plead in defence an *inspiration from Deity*, which so overpowered his mind as to free him from responsibility.

In presenting this last point as a part of the original plan, I have not forgotten that the prosecution distinctly took the ground in the trial that the idea of inspiration was all an after-thought, but from my conversations with Guiteau

during four interviews, I became strongly impressed with the view that the inspiration idea was a part of his original plan. This was confirmed during a fifth interview when, in reply to my question as to when he had *first* told of the inspiration, he said he was *positive* he told Detective Brooks, when he saw him on the night of July 2d, as "it was a distinct part of the idea as it lay in my mind." It was also confirmed by reference to the telegram sent to the *Boston Globe* on July 3d, which contained the same statement, and again by Dr. Gray's testimony on the stand, that Guiteau had admitted that it was a part of his plan at his first or second interview with him in prison.

The above plan exhibits as in full activity, memory, reason, self-control, perception, a full appreciation of the character of his act as it would be regarded by the public, and its immediate effect upon those who should be present, and remarkable judgment in its provision against personal consequences.

Now it is a well-recognized fact that sometimes insane persons make and execute plans, and, therefore, the importance of a plan as a criterion of insanity in a given case is great, chiefly as it may evince defect, or disease, of some faculty of the mind. This is the case generally with the plans of the insane if they are complicated, and they rarely go beyond the accomplishment of the deed, or embrace its consequences so as to provide against them except in an imperfect manner. An insane person who is maniacal, or controlled by delusions, may, for instance, make careful arrangements to escape from an asylum, or to kill his attendant or physician; he may display much cunning in his plan, but he rarely, if ever, goes further than to escape, or shield himself by denial, while all beyond is left to chance; and I think it may be affirmed that never in the history of the insane has one planned the execution of a criminal act in con-

sequence of his insanity, with the *intention beforehand of pleading his insanity as the ground of his irresponsibility.*

The difference, therefore, between the plan of Guiteau and those of the insane, is that it is complicated, and yet clear, connected, and far-reaching in its provisions, and evinces a full appreciation of consequences, while the latter are generally limited or imperfect, do not embrace consequences, and exhibit some impairment of ability to reason, beyond what had previously existed.

Second. We next proceed to the study of *motive.*

That we may be able to understand the significance of motive in any case, it becomes necessary to reason from the standpoint of the actor. The motives for criminal action in different persons are as diverse as actions themselves. It seems improbable in the extreme that a person should break into another's house and kill him for the few paltry dollars he might chance to obtain by so doing; or that a vagabond should burn the house of one who had befriended him, because he had refused to do so longer, and yet the criminal calendar presents the history of many murders and arsons evincing even less ground for motive.

In the case before us, therefore, it becomes necessary to study the motive from the position of Guiteau, in order to understand how much it might signify, and to do this we must refer to his history and movements prior to the act.

It is in evidence that he had years before been interested in a political campaign, and had in mind to secure a mission to a foreign country, had his candidate been successful. In 1880 he had associated himself immediately with the National Committee of the Republican party, and daily visited their headquarters during the campaign. He had received more or less attention from members of that committee, especially the chairman,<sup>1</sup> and his name was down as a

<sup>1</sup> This statement was made to the writer by the Chairman of the Committee.

speaker should his services be called for. The fact that he was daily present at the rooms, and was recognized even in a casual manner by those present, led him to feel that he had a part in conducting the labors which were finally crowned with success, and when the election was over he went on to Washington with the crowd of others who sought office.

His address was good, his appearance was gentlemanly, he could use language fluently, and regarded from his standpoint of view he was as much entitled to an office as a reward for services, or as a personal favor, as others were. Indeed, his heart was set upon it ; he had lost in 1872, but now his party was triumphant, and there could be no failure in his sanguine estimation.

He made his application for the office in due form ; he visited the White House ; he visited the Secretary of State, and was treated with respectful attention. His requests were not granted, but neither were they denied. He was put off with some evasive reply or other, day after day and week after week. In the meantime his condition, financially, was becoming more and more critical ; he was without money or ability to obtain any ; it became necessary to come to some understanding on the matter, and hence his last interview with Mr. Blaine, when he says he was given to understand that if the President would give him the office no objection would be made. He then determined to see the President and have the matter decided once for all ; but an audience was refused, and he resorted to the last means of reaching him, and wrote a letter, but with no effect. His last chance was fading away, and the visions of this office he had so long and so assiduously sought, were becoming more and more dim and hazy to his view ; and all, not because the Secretary of State was unwilling, but because the President was unmindful and ungrateful.

What now is his situation? He is penniless; he has staked his last dollar on securing the mission. He has for months fed upon the crumbs which have fallen from the political table, and upon the words of recognition which have fallen from the lips of those so high above him. There now remains nothing for him but to return to his old and wretched mode of life, wandering about the country selling his book or soliciting insurance, and dead-heading railroads and boarding-houses. How great the contrast as between this and the experience of the last few months; and all, because this miserable, ungrateful President will not give him an office, which he is confident he is as well fitted for, and as much entitled to, as another.

He is a careful reader of the papers, and near this period of his waiting and discouragement, he sees that this author of his misery and disappointment is beginning to be denounced as one who is proving to be ungrateful and unmindful toward the claims of others as well as himself; he waits, and reads, and waits on, and his cause of grievance grows with that of others. This man, who has been raised to the highest position in the gift of the people by the influence of certain men, has turned upon them; he is exhibiting "the blackest ingratitude toward the very men who made him." Certain newspapers denounce him in the severest words; certain men refuse to speak to him even, and some say he is likely to ruin the party; others, that he is the only person in the way of the continued success of "the grand old Republican party," and he is pursuing a course which will destroy it.

He tells us himself that while reading these denunciations of the newspapers and their prophecies of evil, and meditating upon his own disappointments, the idea came to him one night to remove the President. It came to him again and again, and he harbored it; he thought it over;

he read the papers and thought more of it. It would be the removal of that man only who had stood between him and his desires, and had so miserably thwarted them. Others wanted to be rid of him as well as he, or so, at least, he interpreted the newspapers, in the desires and ponderings of his own heart. If it was done, that portion of the party with which he had always been most in sympathy would come into power and office, and he, *he alone*, would be the author of it all. In his greedy imagination which had so long dwelt upon them, these offices must have the same charms for others as for himself, and those who should prove to be so fortunate as to obtain them, and through his act alone, could not fail to condone the means, whatever they might be, by which they obtained them.

Then, his act would at once lift him up to the gaze of the country. The very pistol which he used would ever afterward be exhibited in the national library, and gazed upon by unnumbered thousands, and his name would go "thundering down the ages" as the man who had saved the "grand old Republican party."

Here, then, we have a personal motive of twofold nature: 1, *revenge*; and 2, *notoriety*. The same spirit which had, a short time before, led him to strike his brother in the face for reproving his lying and utterly worthless and dishonest mode of life, led him to strike down the man who had been the cause of his bitter disappointment and failure in securing office. It would be the removal of the man who had dashed to the ground the hopes and expectations of many others as well as his own; and, moreover, also the man whom many of the great leaders of the party never desired to have, and for whom Conkling "did not care that,"—snapping his fingers;—"neither did any of us; he made the fight for Arthur, as we all did."

The same spirit of egotism and love of notoriety which

had led him to wander from one end of the country to the other in imitation of Mr. Moody, that he might keep himself before the public and be the central personage and observed even of a small number of persons, led him in this case also to take the chances. His condition was a desperate one; to drift back once more to the miserable, laborious life he had so long followed, and with such humble results, after he had once tasted the sweets of crumbs from the public table, would be worse to him than even death itself, when it should come in a manner which would so lift him up to the public gaze. Still, as against this last issue of his undertaking he believed he had most carefully provided in his plan. While, therefore, Guiteau had never been guilty of such a monstrous crime before, yet his manner of life, his utter disregard for moral obligations, his yielding to temptation toward evil-doing, his casting off the most sacred social obligations in a criminal manner, and making oath to it in a court of justice, his whole manner of life being one of selfishness and a seeking for notoriety,—all tend to show that for years his course had been one tending toward a condition which prepared him to openly violate law in any of those ways through which temptation might approach him.

Of these two motives I have no doubt the latter one, viz.: love of notoriety, was much the stronger in the mind of Guiteau, though there was enough of the former to lead him on. Moreover, the love of notoriety has little if any influence with the insane. Persons whose brains are so much diseased as to lead to homicide, are not in a condition to be much influenced by it.

But it may be said that the fact that Guiteau had no more appreciation of his unfitness for an important official position was evidence of insanity. In reply I need only remind my readers that lack of intelligence and overestimation of

one's ability are common enough with the sane as well as the insane; ignorance is common alike to both classes of persons; improbable beliefs, incorrect reasoning, eccentricity of conduct, peculiarities of manner and language, claims for public positions by unqualified persons, in this country, are all common enough among the sane, and their importance as criteria of insanity is great only as they may evince degeneration from a condition of greater intelligence and a higher order of conduct at a former period of life; and there is no evidence that Guiteau ever did have any better or higher intelligence.

In view of the above considerations I am unable to find that Guiteau was moved by any other than such definite objects and considerations as operate upon sane minds when they engage in criminal acts.

### III.—*Personal Examination.*

Five months after the shooting of the President, Guiteau was examined several times by me while he was in jail. As the result of these examinations, there were found no indications of important physical disease, or any such malformation of the head as might indicate either insanity or imbecility. There was a peculiarity of facial expression, particularly when excited, which was marked. This I thought due in part or wholly to a slight deviation in the axis of one eye, and pupils rather larger than usual, and also to an inflammation, of a more or less chronic character, of the conjunctivæ.

With reference to the operations of his mind, it may be said that the memory was exceptionally good; he appeared to have a clear and definite remembrance in reference to his family relations, and the details of his own personal history, and of occurrences which took place many years before, and he gave quite a full account of these personal experiences.

He also appeared to have a full and ready comprehension of all questions addressed to him, and of their probable bearing upon his case, not unfrequently guarding, apparently, his replies to them, in a manner favorable to his desire at the time to produce an impression. Any statement or question addressed to him seemed to reach the seat of intelligence at once, and the answer was as quickly sent back. The faculty of perception was well developed and active, and his language well chosen and expressive. There appeared to be neither hallucination nor delusion; neither unnatural excitement, depression, nor dementia. Not once during some eight or nine hours which I spent with him in jail, and the larger portion of which was in conversation, did I notice an imperfect or ungrammatical sentence, or an improper word, or, in any measure, a failure to express easily and fully all that appeared to be in his mind to say on these several occasions.

His statements in reference to the assassination of the President were made readily and as if they had long been thought over by him. He did not hesitate to claim that his act had been occasioned by the conduct of the President, who "had gone back on the very men who made him," and thereby exhibited the "blackest ingratitude" toward them. This statement was made with clenched hand, in a loud tone of voice, and in an excited manner, which appeared as if put on for the occasion.

His whole manner and general bearing appeared to be those of a man in the possession of his will-power and the other faculties of mind, and who intended to persuade not only me, but also the court and the jury, that he had killed the President as a patriotic act, and while *insane*, from a pressure brought to bear upon him by the Deity. This latter point of his case he argued at length, explaining that while he heard no voice of command from the Almighty,

yet he felt a *pressure* to do the deed, and was able finally to work himself up to the point; and that this rendered him irresponsible in law.

He distinctly said he did not think he was now insane, or had been so since five minutes after the shooting, but that if he could make the jury believe that he was insane at that moment, in relation to the act, then they were bound to acquit him. His words as I have them are: "If the jury believe that I believed that I was inspired at that time to do the act, then I was legally insane and they are bound to acquit me." He also referred to the case of General Sickles, which had been tried in that very court-room, as one in which his view of the law had been adopted by the jury, and also the McFarland case, thus showing how carefully he had studied this part of his own case.

When asked what he would have done in case the President had given him the office he sought, he replied, "I suppose that would have ended the matter," though afterward, during the trial in the court-room, he denied that this would have had any effect on his action.

When asked if he should have shot the President on the morning of July 2d if Mrs. Garfield had been with him, as on a former occasion when he refrained from so doing on her account, he replied that he should not, that her presence would have deterred him.

*Conclusion.*—It seems to me that we are warranted in inferring from the above analysis of Guiteau's statements and acts from the 16th of May until the close of the trial, that he was at all times in the possession of his will-power and his natural intelligence; that at no time did he fail to understand that he was preparing to commit an act so highly criminal that he would have been arrested at once if it should become known that he was planning to do it; and that at the time he did commit it he fully understood its

nature, and that he was in the greatest danger of mob violence in consequence of its heinous character. We know that he deliberated long about it before he finally decided, and that judging from his own statements as to plans and motives, he had clear and well-defined ones in his mind ; and though he was mistaken in reference to the effect of his act upon the sentiments of those who he expected would be benefited by it, yet just such results as he fore-saw would come to pass as the result of shooting the President, have actually come to pass, so far as they relate to the Republican party and the general political situation, and also in rendering him one of the most conspicuous of men. We know that on two or more occasions he refrained from shooting the President, and said he should have done so on the fatal morning under certain conditions.

It, therefore, becomes certain that he was not controlled or convulsed by any diseased condition of his nervous system, which destroyed his free agency or his intelligence ; that there was no such general or particular condition of lunacy, that it overpowered any of his mental faculties so that he could not fully control and use them ; but, on the contrary, that he acted with such deliberation, such definiteness of purpose, such a foresight of consequences, both to himself and other persons, and such careful preparation for defence, as evidenced the activity of his mental faculties in their normal condition.

There is no evidence that he has ever experienced any such marked or particular change in the character of his intelligence, or in his conduct, or ability to reason, as indicated disease of brain ; or that he has ever been controlled by any delusions which he could not, and has not abandoned, when he has found them to be impracticable. In view of all the above considerations, I am constrained to come to the conclusion that his act in shooting the President was not the result of a pathological condition of his brain.

It does not, however, follow from the above conclusion that Guiteau is in all respects like other men, or like other great criminals. On the contrary, it seems to me that his general course in life indicates something quite different and exceptional. These peculiarities may perhaps be comprised under the following heads:

1. He has now and has always had an unbalanced brain, *i. e.*, one with its faculties unequally arranged or developed. He has always been greatly egotistical, self-reliant, and sanguine in reference to all that relates to himself. Such characters are not those which usually become insane. The disappointments and failures in plans and purposes of life do not usually harass and depress them, but rather pass over, leaving little effect.

2. While he has some faculties well developed, he has a faulty and weak judgment in reference to his own purposes, convictions, and motives; and also as to the motives and probable course of action in other persons. He also appears to be without that common-sense which enables persons to accurately appreciate the conditions of society, and adjust themselves thereto, so as to get on without friction.

3. While, during the earlier period of life, and under moral influences, he was not specially immoral, and never gave evidence of imbecility in this respect, yet his moral character has never been strong, and the communistic educational influences at the Oneida Community were such as to largely impair what he had. This, together with his subsequent experiences of life, has also served to destroy such feelings of deference and respect toward persons in higher and official positions of life as is common, and hence such unusual exhibitions of conduct as have made his appearance so exceptional.

4. He has had an ambition to be and do more than he

has been willing or able to qualify himself for doing; he has been inconstant of purpose, partly from lack of mental discipline, and partly from innate quality of mind. His desire to be in some conspicuous position appears to have been so boundless as to lead him to place a false estimate on conditions and qualifications requisite, if, indeed, he ever was capable of estimating them.

5. As a consequence of this unfortunate arrangement and development of mental faculties, and still more unfortunate educational influences, and not from disease of brain, he has never been in harmony with the surroundings of his life; he has never been willing to stay in any sphere where he might have succeeded, but has ever been reaching forth, and striving for some position for which he was not and could not be qualified. Failing in all his injudicious plans and visionary schemes of life, he has given more and more a loose reign to his bad impulses, until he has found a home within those darkened walls whence his ambition has a narrow scope.

## NEW BOOKS AND INSTRUMENTS.

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**Diseases of Women.** A Manual for Students and Practitioners. By ARTHUR W. EDIS, M.D., London. Henry C. Lea's Son & Co., Philadelphia, 1882.

This manual presents an excellent summary of the gynecological rules and doctrines considered as indisputable by the group of English and American authors from which it has the air of having been compiled. This statement embraces nearly all that can be said about the book ; for it is difficult to discover in it traces of original opinion, investigation, or experience : knotty points and discussion on debateable subjects are so carefully avoided, that there is little individually characteristic offered to the reviewer for comment. We repeat, it is an excellent manual, clear, decided, sufficiently comprehensive for a beginner, extremely handy for any practitioner, safe, cautious, and precise. The book reminds us constantly of Thomas' treatise, on which it seems to have been somewhat modelled. It contains, however, a special chapter on climacteric disorders, including pseudocyesis, another on hysteria, a third on functional disorders of the bladder, and a fourth on extra-uterine pregnancy,—all subjects left untouched by the American writer. The chapter on hysteria is one of the least satisfactory in the book, the subject being really much too large for the somewhat narrow framework of the author's plan. Dr. Edis admits that for this chapter he is "largely indebted" to Reynolds' article in his "System of Medicine." He quotes, without critical discussion, first, Dr. Barnes, who "insists strongly upon the ovarian theory, proposing indeed to alter the term hysteria to that of "oöphoria"; second, Dr. Reynolds, who "maintains that it has not yet been shown that hysteria has any definite relation to the varying conditions of menstruation"; third, Dr. Grailly Hewitt, who "contends that

when there is an organic cause for hysteria, that cause will be found to be a chronic flexion of the uterus."

Dr. Edis devotes several pages to a description of Dr. Weir Mitchell's rest treatment, but this is taken, not from Mitchell's own writings, but from a paper of Dr. Playfair's on the subject. The author himself has no personal experience or critical judgment to offer in regard to it.

Among the few points in the volume which may suggest special comment, we would notice: The illustration on p. 44 of the "unavoidable" dangers of tents, by a case where a laminaria tent was inserted a few days before menstruation, with a fatal result from peritonitis. This is just the time that tents never should be used, and the author would do better to condemn such a, not unfrequent, recommendation, than to use it as an illustration of "inseparable dangers," which "should deter any but those having special experience in gynecology from resorting to tents." It is probable that at least a considerable number of the accidents caused by tents have occurred in the practice of specialists.

Intra-uterine stems are advocated for the treatment of anteflexions, with perhaps an insufficient warning of their dangers. The special danger attached to their employment in retroflexion, upon which Emmet has justly insisted, is not mentioned. Nothing could be more useful at the present day, than a discussion, supported by statistics, on the relative efficacy of vaginal pessaries and of intra-uterine stems in the treatment of anteflexions; but this discussion perhaps would be out of place in a manual.

We scarcely think that Dr. Barnes is the "only modern author" who has discriminated between fluxion, hyperæmia, congestion, and inflammation, for Courty has been most minute and laborious in establishing distinctions between these different morbid conditions.

Chronic endometritis is said to be, in by far the larger number of cases, "practically incurable." Dr. Routh's distinction of "fundal endometritis," which we believe to be perfectly well founded, is not recognized. The caution to use nitric acid only just after a menstrual period is most necessary, and somewhat remarkable when we remember the unqualified enthusiasm for this medication once excited in Great Britain by Atthill.

We cannot agree with the author that the danger of intra-uterine injections depends on the passage of fluids through the Fallopian tubes. Much more probably do these, especially iron

solutions, penetrate the uterine glands, and thus excite colic. The chapter on fibroids contains descriptions of nearly all methods of operation, but omits Thomas' scoop, and Emmet's peculiar mode of traction. Battey's operation is spoken of very favorably, as offering "great hope," not only to sufferers from uterine fibroids, but "to a large number of women who suffer from ovarian disorders, attended with nervous manifestations of the most distressing character."

Dr. Edis' book will not help those who are searching for such profound and original suggestions as may, one day, furnish the guiding clue through the present labyrinth of gynecology. But as a summary of existing knowledge, empirical and other, it is really to be commended.

[M. P. J.]

**Lectures on Diseases of Children.** A Handbook for Physicians and Students. By Dr. EDWARD HENOCH, Professor in the University of Berlin. New York: Wm. Wood & Co., 1882.

This excellent translation of the last edition of Henoch's lectures is really an admirable contribution to Wood's Library of Standard Medical Authors. It offers a marked contrast to the treatise by the English Dr. Ellis, issued a few years ago in an American edition by the same publishers. It contains in a compact form a very large amount of the most recent and authoritative information on paediatrics, and this is stated so concisely, that the reader may easily fail to perceive how much has been told to him, for the very reason that it has been told so well. The author bases his lectures exclusively on his own experience; but as this is very large, he is able to cite cases illustrating not only classical and regular forms of disease, but the rare, often extremely rare, clinical varieties. Indeed, the description of individual cases is almost always devoted to the illustration of such rarities. These clinical histories are remarkable for their conciseness, and appropriateness to the point at issue.

The lectures being exclusively clinical, contain only brief directions on the hygiene and dietetics of infancy, such as usually open systematic treatises on paediatrics. The first chapter, after a general introduction, is devoted to diseases of the new-born. The nature of icterus neonatorum is somewhat fully discussed; but nothing is said of the generalized fatty degeneration, of which a malignant form of icterus may be one of the symptoms. Trismus neonatorum is regarded, "like epilepsy, as a spasmodic disease,

specific only in appearance, and which can be produced by various irritants." Thus, baptism in cold water, hot baths administered by a "busy midwife, unable to distinguish a temperature of 33° R. from 28°" have both been causes of trismus. A case is cited where the trismus developed on the 13th day, with nephritis, the umbilicus being normal. Henoch admits two forms of erysipelas in the new-born, of which only one is related to puerperal infection. The traumatic form may start not only from the navel, but from red excoriations about the nates and genitals. The occurrence of abcesses, though indicating great danger, is not necessarily fatal. We ourselves have observed a case in which after complete convalescence and disappearance of the eruption, a six months' child suffered from a succession of subcutaneous abcesses unattended by pain, heat, or redness of the integument, and hardly disturbing the euphoria. The chapter on infantile syphilis lays due emphasis on its osseous affections. The author does not recognize any difference in the form of the syphilitic dactylitis from that of the scrofulous paedarthrocace; even the epiphyseal swellings of rachitis are only to be distinguished by their constant symmetry.

The remarkable phenomenon of complete immobility of the limbs affected by syphilitic osteomyelitis, is carefully described. Bednar, the "first author who attached any importance to this symptom," ascribed it to a myopathic affection. But Henoch explains the immobility by the pain. Cases are adduced of paresis without any discernible osseous affection, and these are explained by Wegner's discovery of a peculiar lesion in the formative zone lying between the epiphysis and diaphysis. There is excessive proliferation of cartilage cells, retarded ossification of the already calcified tissue, retarded development of vessels, with consequent deficient nutrition of cells, their shrinkage, fatty degeneration and ultimate necrosis of tissue. Waldeyer and Kobner think this process is gummatous, and that the necrosis results from compression of vessels through an excessive cell proliferation. The important clinical fact remains, that there is a diseased process at the boundary of the epiphysis, which may markedly affect the mobility of the limbs, without betraying itself by either swelling or pain.

One of the most interesting chapters in the book is that on diseases of the nervous system. Like the others it is predominantly clinical, and contains no elaborate discussions on infantile peculiarities in the anatomy or physiology of nervous organs. Many interesting clinical facts are related, especially in the section on

the usually neglected subject of hysterical affections. Among these, Henoch makes a first category of cases which many authors would certainly regard as epileptiform. "Cases in which the psychical symptoms, viz.: complete or incomplete loss of consciousness, hallucinations, delirium, predominate. \* \* \* Consciousness is suddenly lost, the children remain standing or sitting with a fixed stare; occasionally they fall if not supported. \* \* \* After a few seconds, at the most several minutes, every thing is over, and the patient entirely restored. Many are unaware that they have had an attack." The author admits that the physician "can never be certain that these attacks will not degenerate into epilepsy": but it seems to us that those in which consciousness is lost may be already diagnosticated as epileptoid. Cases of hallucinations and delirium, lasting more or less continuously for two or three days, are also called hysterical. We have seen one case of prolonged liability to hallucinations and excitement, accompanied by persistent mutism, where the diagnosis, made by several neurologists, hesitated between incipient tubercle of the brain, true mania, and worms!

In a second category of hysterical affections, Henoch includes those cases in which convulsive symptoms predominate, either general or local, and those especially affecting the muscles of phonation. A third category is formed of the remarkable cases known as chorea magna. One extraordinary case of this disease is related, occurring in a girl eleven years old. It lasted five years, and exhibited the most "varied manifestations of changed nervous actions, viz.: psychical disturbances, hallucinations, and delirium, jumping and running paroxysms, opisthotonus, choreic movements, partial hyperæsthesia, and a sort of clairvoyance by which the patient was enabled to foretell the number of 'sounds' and their change of type."

Paroxysms of spasms, motions, attacks of intense local hyperæsthesia, in one case hematemesis, are reported in various clinical histories. The hematemesis was observed in a girl of eleven, prematurely developed, but not yet menstruated. It occurred on alternate nights at the same hour for ten days, each attack lasting half an hour. Henoch, having excluded simulation, having observed the occurrence of one of these attacks in the daytime, under the influence of emotional excitement, and their disappearance by purely moral treatment, ascribes them to "an irritation of the dilator nerves" of the gastric blood-vessels.

Such periodical congestions are often attributed to malarial

poisoning. Perhaps, when this etiological factor is absent, the proximate process in hysterical hyperæmias is nevertheless identical with that induced by malaria. At all events, the clinical resemblance between hysterical affections and latent or "dumb" ague is often extraordinary.

Spasmus nutans as a benign disease, due to a reflex irritant, is differentiated from the "dangerous variety of undoubtedly central origin," described by Newnham and Willshire. We had hoped to perhaps find in this chapter, reference to a form of lateral rotatory movements, occurring exclusively during sleep, instead of being arrested by it; rare symptom-complex, of which we have observed one case. But Henoch, as well as the other systematic writers we have consulted, fails to mention this.

In the etiology of convulsions, rickets is held, we believe justly, to play a far more important part than dentition. Idiopathic contractures of the extremities are regarded as abortive convulsions, and to be entirely distinguished from tetany. They can never be produced by pressure upon the artery supplying the affected limb. The author denies the connection between spasm of the glottis and crano tabes; and while recognizing a manifest relation between this neurosis and rachitis, considers the relation as at present inexplicable. He gives an interesting description of certain forms of pertussis paroxysm, consisting in attacks of apnoea that somewhat simulate spasmus glottidis, and which may easily prove fatal. We have recently observed precisely such a case, in a twin baby five months old, and who did succumb in an attack.

Only a paragraph is devoted to "spastic spinal paralysis," the "tetanoid paraplegia" of Erb and Seguin. It is decided to be little more than a symptom-complex, not corresponding to any definite anatomical change. "The frequently observed complication with slight mental development, even with idiocy, permits the conclusion that similar symptoms may start from the brain"; and a case is given where the autopsy showed marked structural changes of the cerebral cortex. In such cases secondary degeneration is to be expected in the cord.

A constant spinal lesion is denied as the cause of pseudo-hypertrophy of the muscles.

In the chapters on cerebral tuberculosis we find the author insisting on the frequent latency of the lesions, and on the feeble basis for regional diagnosis of tubercles. "I have often seen hemiplegia and contractures in cases in which the

autopsy showed that the corresponding parts of the brain were entirely intact, while various other parts of the cerebrum or cerebellum were the site of tubercles." Cerebral tuberculosis is held to be capable of spontaneous recovery, unless meningo-encephalitis has set in, when the case becomes hopeless. Chronic hydrocephalus is by no means always due to tubercle ; nor even always to a chronic inflammation of the ependyma of the ventricles ; but we are sometimes driven to the "unsatisfactory assumption of a fault in development, an excessive secretion of cerebro-spinal fluid." The rolling downward of the eyes in this disease is due to paralysis of the nerve supplying the superior rectus muscle, with consequent predominance of the inferior rectus, and not to presence of fluid which should produce, if anything, exophthalmus.

Henoch believes decidedly in the existence of a primary non-diphtheritic croup, "the highest development of acute laryngitis." This idiopathic croup is susceptible of recovery without tracheotomy ; but a case is cited of death in a boy of eight, nine days after subsidence of the laryngitis, from venous stasis in the cerebral veins, oedema of the pia mater, and effusion into the ventricles. In prolonged broncho-pneumonia, and also in pertussis, the author has several times seen fatty degeneration of the heart, produced by the great resistance offered to the right ventricle. He does not refer to the ingenious remark of Baginsky, that in the pulmonary inflammations of childhood, the increased resistance offered to the right ventricle is less dangerous than in adult life, because it varies less widely from the conditions normal to the age. In children the calibre of the pulmonary artery much more nearly approaches that of the aorta, than is the case in adults ; the tension in the pulmonary circulation is, relatively to that of the systemic, much higher. The right heart is therefore habituated to bear more relative strain, a circumstance which helps to explain the favorable prognosis of pneumonias in childhood. There is much more danger from their chronicity and the tendency to engorgement and cheesy degeneration of the bronchial glands, than from the cardiac paralysis which is so threatening in adult life.

In the very brief chapter on gastric dyspepsia the author refers to a form of reflex disturbance which he had previously described as "asthma dyspepticum." In an illustrative case a girl of nine began suddenly to have cyanosis : respiration of 70, moaning expiration, superficial movement of alæ nasi ; pulse small, 108. All the symptoms disappeared after the spontaneous vomiting of a hard-boiled egg.

In the chapter on tubercular peritonitis, an interesting case shows: 1st, that a severe chronic form of this disease may exist without any pain, and merely manifest itself by the symptoms of ascites, increasing emaciation, and cachexia; 2d, that then enlargement of the liver from fatty infiltration may confuse the diagnosis. A case under our own observation confirms both these propositions. It is rather unusual to find in lectures on paediatrics, a separate chapter reserved for diseases of the spleen, Henoch describes a simple hypertrophy of this organ, only rarely preceded by intermittent fever, and unaccompanied either by the blood changes of true, or the multiple adenitis of the pseudo-leukæmia. Recovery is not uncommon.

One of the most interesting observations in connection with nephritis, is that of anasarca without albuminuria, or sign of morbid elements in the urine during several days; then, coinciding with the development of uræmic symptoms, the urine drawn by the catheter was found abundantly albuminous, and the autopsy disclosed an exquisite parenchymatous nephritis. The author regards these cases as at present inexplicable. It seems to us that they can be explained by a congestion of the glomeruli sufficient to seriously diminish the excretion of water and thus account for its surplus retention and effusion into cellular tissues; while, nevertheless, the epithelium of the capsule had not yet desquamated, and therefore no albumen could escape into the tubes.

We might continue to make many other extracts from these compact lectures, but space forbids. The therapeutical sections are strongly tinged with the nihilism so prevalent in Germany, balanced, as is so often the case, by what we should consider an undue partiality for calomel. This is given even in the dyspepsia of infants, for its antifermentative action. We can see no reason for preferring calomel to gray powder in any disease of childhood, except syphilis, or, possibly, acute, simple laryngitis. Iron, cod-liver oil, and, rather strangely, morphine, are about the only remedies advised for the entire list of nervous disorders. Even chorea is held to resist all treatment, even that by arsenic,—an opinion certainly unfounded. The treatment of pneumonia, either catarrhal or croupous, is either expectant, or much more powerfully anti-phlogistic than is at present usually recommended. We think in both lines of treatment the author is influenced by his rather excessively favorable prognosis in this disease. In vigorous children he applies not only dry but wet cups and leeches; and even advises, "when careful watching is possible," the justly

abandoned tartar emetic. There is no reason for selecting this instead of the far more manageable kermes mineral. Henoch has given up quinine in croupous pneumonia, and confines himself to local applications of cold, infus. digitalis, and kali nitricum. He lays great stress on the value of the constant cold compress, and does not seem to have compared it with the warm poultice and warm bath. For the paroxysms of pertussis, he advises only morphine, pronounces atropine too dangerous, and seems to have made no trial of belladonna,—a most excellent remedy. From the treatment of scarlatina is omitted the use of inunctions. The danger of cold baths is justly insisted upon, as also the fact, calculated to lessen intemperate antipyretics, that cardiac paralysis is threatened as much from the scarlatinous virus as from high temperature. But the amount of stimulation advised in this disease, and more especially in diphtheria, seems to us altogether inadequate.

The simplicity of children's diseases, and of their symptoms, justifies the effort to simplify their treatment; and it is certainly desirable that a severe rationalism should purge this of the polypharmacy of a former generation. But we believe the indications are more varied than those laid down by Henoch, and that more successes can be scored than he seems willing to admit.

One other remark. It seems to us a great misfortune that the orthopedics of childhood should be habitually separated from the medical treatises of their diseases, and consigned to special essays, or to works on surgery. Although mechanical principles are involved in the treatment of such diseases, the important question of their early recognition and diagnosis presents itself as a practical problem in the daily experience of every family physician. The habit of referring these diseases to the specialist or to the surgeon, is responsible for much of their neglect by these same practitioners, who, in consulting systematic lectures on children's diseases, accustom themselves to expect from them at least an outline of all the physician need be called upon to know.

[M. P. J.]

**A Treatise on the Science and Practice of Medicine, or the Pathology and Therapeutics of Internal Diseases.** By ALONZO B. PALMER, M.D., LL.D., Professor of Pathology and Practice of Medicine, and of Clinical Medicine in the University of Michigan; formerly Professor in the Berkshire Medical College, Mass., and in the Bowdoin Medical College, Maine, etc., vol. i. New York : G. P. Putnam's Sons, 1882.

It has long been well recognized that the *types of the people* have a determining influence in causing variation in the manifestations of the same disease. The identical disease on the Continent frequently contrasts, rather than compares in its symptoms and in its course, when appearing in America. Within our own country even, embracing such varying climates, with its Northern cold and its Southern tropics, its new lands of the West and its old and over-crowded cities of the East, we meet with diseases in one section, which are either comparatively unknown or present marked difference in another. The appearance of American works on Practice of Medicine has been justified by the intention of their authors to present these views, and to call attention to the necessary accompanying variation in treatment, which should go with change in the type of a disease.

Based upon an experience in country practice exceptionally large, and combined with that in cities, our author has found himself in a position in which he states he can present views and direct treatment for those of his brethren who shall follow more or less in his footsteps in a country or far Western practice, which shall serve them more immediately and directly, than the foreign, or perhaps other American, works. The book before us comprises the first volume of the work. It contains, first, general considerations on pathology; then a discussion of "particular general diseases," under which are included symptomatic fevers, exanthemata, malarial affections, leucocythemia, diabetes, etc. Next follow the local diseases, and of these, those affecting the alimentary canal, the pancreas, spleen, and liver are discussed.

The general pathological observations are brief, and are based upon views held in Flint's work, and to which due credit is given. But neither these, nor the mode of presentation of the pathology, as given in any portion of this work, constitute in any way its strong feature. On the contrary, when, as on page 221, in speaking of changes in the blood, we read such general remarks as, "the blood in some cases has been found disorganized," we quote a vagueness of expression not infrequently found in this connection. It is in the thorough familiarity with disease in its protean types that our author finds his claim, not only to our respectful attention, but even admiration. Coming from this particular source we would naturally expect to see ague, one of the common affections of the far West, treated at some length and in detail; and we are pleased to note some sixteen readable pages on the treatment alone of this affection. We cannot help quoting some

remarks which, we think, should be carefully thought over by our readers. One that "cinchonism is no adequate measure of the curative effect of quinine," since minute doses produce cinchonism in some people. The practice of giving small and long-continued doses of quinine in ague is bad. Prompt and large effectual doses in ague, just as when quinine is used as an antipyretic, is strongly urged.

Among the febrile disorders we find miliary fever discussed, which, as our author states, is little if at all referred to in other works on practice. This article is based on Zuelzer's article in Ziemssen's Cyclopædia, from Grisolle and Jaccoud's recent works on practical medicine, and from personal statements of physicians in Rome made to the author.

Miliary fever is characterized as a specific disease, produced by a peculiar poison, but that this poison does not originate in the body, and is not multiplied in it in a manner to be communicated to others, or, at least, this is not its principal method of production and spread; that it is a disease of variable severity and course, and of uncertain termination. Its general characters, in addition to a prolonged febrile movement, are repeated sweating and the appearance of a marked sudaminous eruption. But, before concluding his article, we find that the existence of miliary fever as a distinct disease is doubted, and that sudamina and sweating are regarded as epiphenomena of certain cases of typhoid fever.

In the discussion of diabetes mellitus, which is fully given, reference is made to all the most important views in connection with the subject. Fat embolism is referred to; the subject of acetonæmia, or death by diabetic coma, being, however, omitted. A variety of rheumatism, known as neuralgic, not generally alluded to, is included. In the general treatment, the combinations of the cathartic, alkaline, and salicylic-acid treatment find most favor, and when so conjoined, are thought to be almost specific. An enjoyable chapter is found on dysentery, with its treatment considered at some length, and the use of quinine recommended as an antiphlogistic in these cases. In the treatment of peritonitis the author makes a statement to which our own experience, and the advice of Dr. Alonzo Clark, to whom the credit of the opium treatment in this disease belongs, run contrary. It is urged, when using opium or morphine, to diminish the frequency of the respirations, but not greatly. As it is thought advisable to reduce respirations, down to 12 or 15, and even 10 per minute, from 40 or 60 per minute, such a reduction must, in our opinion, be

viewed as decided. In concluding our review of the first volume, we can say that it will appeal to the class of readers for whom it was intended, and that it abounds throughout with practical observations. The student and practitioner who seek a work in which they shall find, instead of descriptions of rare pathological conditions, clear expositions of disease, symptoms, and conditions of daily occurrence, with excellent recommendations as to their treatment, will be fully satisfied by this.

[H. N. H.]

## ORIGINAL OBSERVATIONS.

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### CASE OF CEREBRAL TUMOR ILLUSTRATING THE DOCTRINE OF LOCALIZATION.

By WM. S. CHEESMAN, M.D.,

AUBURN, N. Y.

Patient, a woman aged 42. No history of injury or syphilis. She had long been subject to headaches. In January last she began to lose power over her right hand and arm, and on February 18th she completely lost the use of the hand and arm, yet did part of her work as domestic. On February 21st she attended a prayer-meeting, and there was seized with a convulsion. On recovering consciousness she was hemiplegic on the right side. She was admitted to the Auburn City Hospital February 22d, with right hemiplegia and dysphasia, though her mind seemed clear. Her condition so far improved that the paralysis of the lower limb became paresis, while that of the face disappeared. More or less contracture of the hand and arm supervened. She gradually became duller—had involuntary evacuations, vomited considerably, had several convulsions, and finally died in coma on April 11, 1882. No ophthalmoscopic examination was made.

*Autopsy* two hours and a half after death.

Nothing abnormal in organs of chest or abdomen.

Skull, meninges, and surface of the brain normal.

After removing the brain, a bulging was noted at the top of the ascending frontal convolution of the left hemisphere, the ascending parietal convolution being pushed backward to make room for it. To the touch this was very soft. A longitudinal section through it exposed a rounded mass of the size of an English walnut, reddish brown in color, soft and friable—the cortex

covering it being swollen and gelatinous. The section also revealed in the white substance beneath and behind this mass, a second of the same size, its lower border nearly reaching the roof of the left lateral ventricle. These were separated by a narrow partition of compromised white substance.

The right hemisphere was normal.

A microscopic examination of the specimens was made by Prof. W. H. Welch of Bellevue Hospital Medical College. In his opinion the growths were nerve-cell gliomata.



FIG. 1.—Location of tumor as indicated upon an Ecker's diagram of the convexity of the brain.



FIG. 2.—Longitudinal vertical section of the left hemisphere, showing the location of the two tumors.

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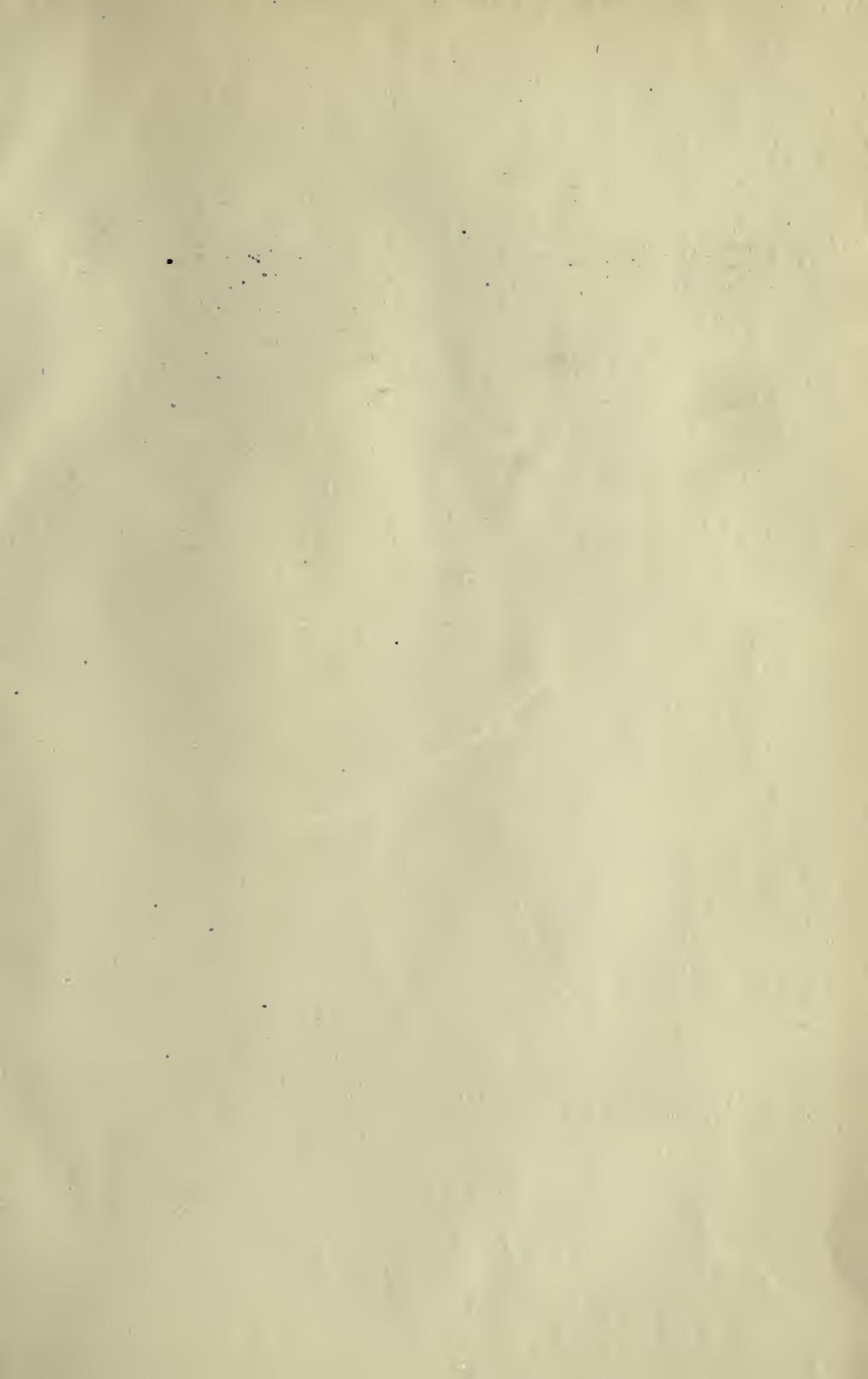
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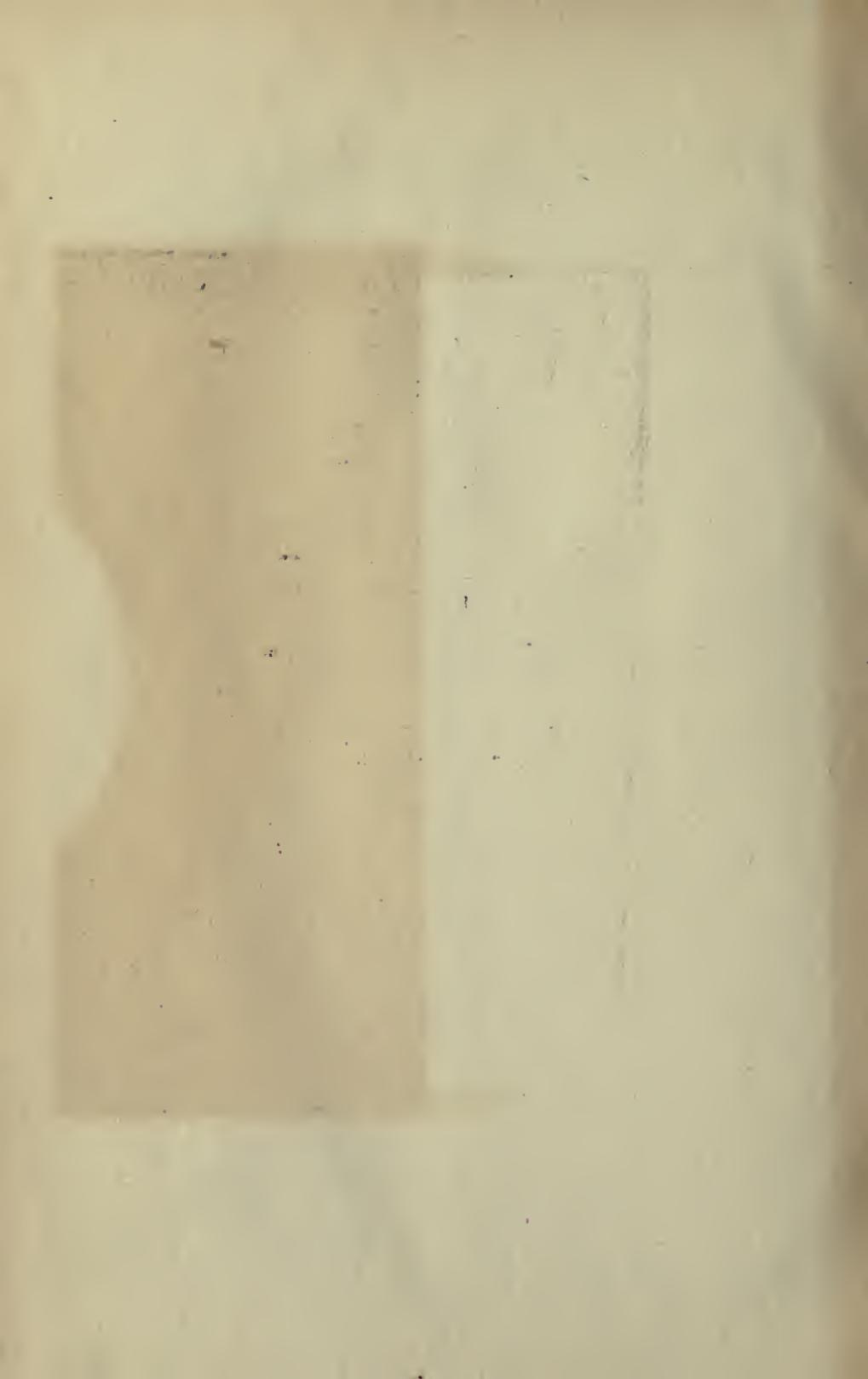
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